NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

Royal Institution of Great Britain,

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS.

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PLATE.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 21, 1887.

WILLIAM HUGGINS, Esq. D.C.L. LL.D. F.R.S. Manager and Vice-President, in the Chair.

SIR WILLIAM THOMSON, LL.D. F.R.S. *M.R.I.*

PROFESSOR OF NATURAL PHILOSOPHY IN THE UNIVERSITY OF GLASGOW.

The Sun's Heat.

FROM human history we know that for several thousand years the sun has been giving heat and light to the earth as at present, possibly with some considerable fluctuations, and possibly with some not very small progressive variation. The records of agriculture, and the natural history of plants and animals within the time of human history, abound with evidence that there has been no exceedingly great change in the intensity of the sun's heat and light within the last three thousand years; but for all that, there may have been variations of quite as much as 5 or 10 per cent., as we may judge by considering that the intensity of the solar radiation to the earth is $6\frac{1}{2}$ per cent. greater in January than in July; and neither at the equator nor in the northern or southern hemispheres has this difference been discovered by experience or general observation of any kind. But as for the mere age of the sun, irrespective of the question of uniformity, we have proof of something vastly more than three thousand years in geological history, with its irrefragable evidence of continuity of life on the earth in time past for tens of thousands, and probably for millions of years.

Here, then, we have a splendid subject for contemplation and research in Natural Philosophy or Physics—the science of dead matter. The sun, a mere piece of matter of the moderate dimensions which we know it to have, bounded all round by cold ether,* has been doing work at the rate of four hundred and seventy-six thousand million million million horse-power for three thousand years; and at

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* The sun warms and lights the earth by wave motion, excited in virtue of his white-hot temperature, and transmitted through a material commonly called the luminiferous ether, which fills all space as far as the remotest star, and has the property of transmitting radiant heat (or light) without itself becoming heated. I feel that I have a right to drop the adjective luminiferous, because the medium, far above the earth's surface, through which we receive sun-heat (or light), and through which the planets move, was called ether 2000 years before chemists usurped the name for "sulphuric ether," "muriatic ether," and other compounds, fancifully supposed to be peculiarly ethereal; and I trust that chemists of the present day will not be angry with me if I use the word ether, pure and simple, to denote the medium whose undulatory motions constitute radiant heat (or light).

possibly a higher, certainly not much lower, rate for a few million years. How is this to be explained? Natural philosophy cannot evade the question, and no physicist who is not engaged in trying to answer it can have any other justification than that his whole working time is occupied with work on some other subject or subjects of his province by which he has more hope of being able to advance science.

It may be taken as an established result of scientific inquiry that the sun is *not* a burning fire, and *is* merely a white hot fluid mass cooling, with some little accession of fresh energy by meteors occasionally falling in, but of very small account in comparison with the whole energy of heat which he gives out from year to year. Helmholtz's form of the meteoric theory of the origin of the sun's heat, may be accepted as having the highest degree of scientific probability that can be assigned to any assumption regarding actions of prehistoric times. The essential principle of the explanation is this; at some period of time, long past, the sun's initial heat was generated by the collision of pieces of matter gravitationally attracted together from distant space to build up his present mass; and shrinkage due to cooling gives, through the work done by the mutual gravitation of all parts of the shrinking mass, the vast heat-storage capacity in virtue of which the cooling has been, and continues to be, so slow.

In some otherwise excellent books it is "paradoxically" stated that the sun is becoming hotter because of the condensation.* Paradoxes have no place in science. Their removal is the substitution of true for false statements and thoughts, not always so easily effected as in the present case. The truth is, that it is because the sun is becoming less hot *in places of equal density* that his mass is allowed to yield gradually under the condensing tendency of gravity and thus from age to age cooling and condensation go on together.

An essential detail of Helmholtz's theory of solar heat is that the sun must be fluid, because even though given at any moment hot enough from the surface to any depth, however great, inwards, to be brilliantly incandescent, the conduction of heat from within through solid matter of even the highest conducting quality known to us, would not suffice to maintain the incandescence of the surface for more than a few hours, after which all would be darkness. Observation confirms this conclusion so far as the outward appearance of the

* [Note of February 21, 1887.—The "paradox" referred to here, is, as I now find, merely a mis-statement (faulty and manifestly paradoxical through the omission of an essential condition) of an astonishing and most important conclusion of a paper by J. Homer Lane, which appeared in the 'American Journal of Science, for July 1870 (referred to more particularly on p. 11 below). In Newcomb's 'Popular Astronomy,' first edition, p. 508, the omission is supplied in a footnote, giving a clear popular explanation of the dynamics of Lane's conclusion; and the subject is similarly explained in Ball's 'Story of the Heavens,' pp. 501, 502, and 503, with complete avoidance of the "paradox." And now I take this opportunity of correcting my hasty correction of the "paradox" by the insertion of the five words in italics added to line 6 of the paragraph.—W. T.]

sun is concerned, but does not suffice to disprove the idea which was so eloquently set forth by Sir John Herschel, and which prevailed till thirty or forty years ago, that the sun is a solid nucleus inclosed in a sheet of violently agitated flame. In reality, the matter of the outer shell of the sun, from which the heat is radiated outwards, must in cooling become denser, and so becoming unstable in its high position must fall down, and hotter fluid from within must rush up to take its place. The tremendous currents thus continually produced in this great mass of flaming fluid constitute the province of the newly-developed science of solar physics, which, with its marvellous instrument of research—the spectroscope—is yearly and daily giving us more and more knowledge of the actual motions of the different ingredients, and of the splendid and all-important resulting phenomena.

To form some idea of the amount of the heat which is being continually carried up to the sun's surface and radiated out into space, and of the dynamical relations between it and the solar gravitation, let us first divide that prodigious number (476×10^{21}) of horse-power by the number ($6 \cdot 1 \times 10^{18}$) of square metres * in the sun's surface, and we find 78,000 horse-power as the mechanical value of the radiation per square metre. Imagine, then, the engines of eight ironclads applied, by ideal mechanism of countless shafts, pulleys, and belts, to do all their available work of, say 10,000 horse-power each, in perpetuity driving one small paddle in a fluid contained in a square metre vat. The same heat would be given out from the square metre * surface of the fluid as is given out from every square metre of the sun's surface.

But now to pass from a practically impossible combination of engines, and a physically impossible paddle and fluid and containing vessel, towards a more practical combination of matter for producing

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* A square metre is about $10\frac{3}{4}$ (more nearly 10·764) square feet, or a square yard and a fifth (more nearly 1·196 square yards). The metre is a little less than 40 inches (39·37 inches = 3·281 feet = 1·094 yards). The kilometre, which we shall have to use presently, being a thousand metres, is a short mile as it were (·6214 of the British statute mile). Thus in round numbers 62 statute miles is equal to 100 kilometres, and 161 kilometres is equal to 100 statute miles. The awful and unnecessary toil and waste of brain power involved in the use of the British system of inches, feet, yards, perches, or rods, or poles, "chains," furlongs, British statute miles, nautical miles, square rod ($30\frac{1}{4}$ square yards)! rood (1210 square yards)! acre (4 roods), may be my apology, but it is only a part of my reason, for not reckoning the sun's area in acres, his activity in horse-power per square inch or per square foot, and his radius, and the earth's distance from him in British statute miles, and for using exclusively the one-denominational system introduced by the French ninety years ago, and now in common use in every civilised country of the world, except England and the United States of North America. The British ton is 1·016 times the French ton, or weight of a cubic metre of cold water (1016 kilogrammes). The French ton, of 1000 kilogrammes, is ·9842 of the British ton. Thus for many practical reckonings, such as those of the present paper, the difference between the British and the French ton may be neglected.

the same effect: still keep the ideal vat and paddle and fluid, but place the vat on the surface of a cool, solid, homogeneous globe of the same size (697,000 kilometres radius) as the sun, and of density (1·4) equal to the sun's mean density. Instead of using steam-power, let the paddle be driven by a weight descending in a pit excavated below the vat. As the simplest possible mechanism, take a long vertical shaft, with the paddle mounted on the top of it so as to turn horizontally. Let the weight be a nut working on a screw-thread on the vertical shaft, with guides to prevent the nut from turning—the screw and the guides being all absolutely frictionless. Let the pit be a metre square at its upper end, and let it be excavated quite down to the sun's centre, everywhere of square horizontal section, and tapering uniformly to a point in the centre. Let the weight be simply the excavated matter of the sun's mass, with merely a little clearance space between it and the four sides of the pit, and with a kilometre or so cut off the lower pointed end to allow space for its descent. The mass of this weight is 326 million tons. Its heaviness, three-quarters of the heaviness of an equal mass at the sun's surface, is 244 million tons solar surface-heaviness. Now a horse-power is, per hour, 270 metre-tons, terrestrial surface-heaviness; or 10 metre-tons, solar surface-heaviness, because a ton of matter is twenty-seven times as heavy at the sun's surface as at the earth's. To do 78,000 horse-power, or 780,000 metre-tons solar surface-heaviness per hour, our weight must therefore descend at the rate of one metre in 313 hours, or about 28 metres per year.

To advance another step, still through impracticable mechanism, towards the practical method by which the sun's heat is produced, let the thread of the screw be of uniformly decreasing steepness from the surface downwards, so that the velocity of the weight, as it is allowed to descend by the turning of the screw, shall be in simple proportion to distance from the sun's centre. This will involve a uniform condensation of the material of the weight; but a condensation so exceedingly small in the course even of tens of thousands of years, that, whatever be the supposed material, metal or stone, of the weight, the elastic resistance against the condensation will be utterly imperceptible in comparison with the gravitational forces with which we are concerned. The work done per metre of descent of the top end of the weight will be just four-fifths of what it was when the thread of the screw was uniform. Thus, to do the 78,000 horse-power of work, the top end of the weight must descend at the rate of 35 metres per year: or 70 kilometres per 2000 years.

Now let the whole surface of our cool solid sun be divided into squares, for example as nearly as may be of one square metre area each, and let the whole mass of the sun be divided into long inverted pyramids or pointed rods, each 697,000 kilometres long, with their points meeting at the centre. Let each be mounted on a screw, as already described for the long tapering weight which we first considered; and let the paddle at the top end of each screw-shaft revolve

in a fluid, not now confined to a vat, but covering the whole surface of the sun to a depth of a few metres or kilometres. Arrange the viscosity of the fluid and the size of each paddle so as to let the paddle turn just so fast as to allow the top end of each pointed rod to descend at the rate of 35 metres per year. The whole fluid will, by the work which the paddles do in it, be made incandescent, and it will give out heat and light to just about the same amount as is actually done by the sun. If the fluid be a few thousand kilometres deep over the paddles, it would be impossible, by any of the appliances of solar physics, to see the difference between our model mechanical sun and the true sun.

To do away with the last vestige of impracticable mechanism in which the heavinesses of all parts of each long rod are supported on the thread of an ideal screw cut on a vertical shaft of ideal matter, absolutely hard and absolutely frictionless: first, go back a step to our supposition of just one such rod and screw working in a single pit excavated down to the centre of the sun, and let us suppose all the rest of the sun's mass to be rigid and absolutely impervious to heat. Warm up the matter of the pyramidal rod to such a temperature that its material melts and experiences as much of Sir Humphry Davy's "repulsive motion" as suffices to keep it balanced as a fluid, without either sinking or rising from the position in which it was held by the thread of the screw. When the matter is thus held up without the screw, take away the screw or let it melt in its place. We should thus have a pit from the sun's surface to his centre, of a square metre area at the surface, full of incandescent fluid, which we may suppose to be of the actual ingredients of the solar substance. This fluid, having at the first instant the temperature with which the paddle left it, would at the first instant continue radiating heat just as it did when the paddle was kept moving; but it would quickly become much cooler at its surface, and to a distance of a few metres down. Currents of less hot fluid tumbling down, and hotter fluid coming up from below, in irregular whirls, would carry the cooled fluid down from the surface, and bring up hotter fluid from below, but this mixing could not go on through a depth of very many metres to a sufficient degree to keep up anything approaching to the high temperature maintained by the paddle; and after a few hours or days, solidification would commence at the surface. If the solidified matter floats on the fluid, at the same temperature, below it, the crust would simply thicken as ice on a lake thickens in frosty weather; but if, as is more probable, solid matter, of such ingredients as the sun is composed of, sinks in the liquid when both are at the melting temperature of the substance, thin films of the upper crust would fall in, and continue falling in, until, for several metres downwards, the whole mass of mixed solid and fluid becomes stiff enough (like the stiffness of paste or of mortar) to prevent the frozen film from falling down from the surface. The surface film would then quickly thicken, and in the course of a few hours or days become less than

red-hot on its upper surface, the whole pit full of fluid would go on cooling with extreme slowness until, after possibly about a million million million years or so, it would be all at the same temperature as the space to which its upper end radiates.

Let precisely what we have been considering be done for every one of our pyramidal rods, with, however, in the first place, thin partitions of matter impervious to heat separating every pit from its four surrounding neighbours. Precisely the same series of events as we have been considering will take place in every one of the pits.

Suppose the whole complex mass to be rotating at the rate of once round in twenty-five days, which is, about as exactly as we know it, the time of the sun's rotation about his axis.

Now at the instant when the paddle stops let all the partitions be annulled, so that there shall be perfect freedom for currents to flow unresisted in any direction, except so far as resisted by the viscosity of the fluid, and leave the piece of matter, which we may now call the Sun, to himself. He will immediately begin showing all the phenomena known in solar physics. Of course the observer might have to wait a few years for sunspots, and a few quarter-centuries to discover periods of sunspots, but they would, I think I may say probably, all be there just as they are, because I think we may feel that it is most probable that all these actions are due to the sun's own substance, and not to external influences of any kind. It is, however, quite possible, and indeed many who know most of the subject think it probable, that some of the chief phenomena due to sunspots arise from influxes of meteoric matter circling round the sun.

The energy of chemical combination is as nothing compared with the gravitational energy of shrinkage, to which the sun's activity is almost wholly due. A body falling forty-six kilometres to the sun's surface *or through the sun's atmosphere*, has as much work done on it by gravity, as corresponds to a high estimate of chemical energy in the burning of combustible materials. But chemical combinations and dissociations may, as urged by Lockyer, in his book on the 'Chemistry of the Sun,' just now published, be thoroughly potent determining influences on some of the features of non-uniformity of the brightness in the grand phenomena of sun-spots, hydrogen flames, and corona, which make the province of solar physics. But these are questions belonging to a very splendid branch of solar science to which only allusion can be made at the present time.

What concerns us as to the explanation of sun-light and sun-heat may be summarised in two propositions:—

(1) Gigantic currents throughout the sun's liquid mass are continually maintained by fluid, slightly cooled by radiation falling down from the surface, and hotter fluid rushing up to take its place.

(2) The work done in any time by the mutual gravitation of all the parts of the fluid, as it shrinks in virtue of the lowering of its temperature, is but little less than (so little less than, that we may regard it as practically equal to) the dynamical equivalent of the heat that is radiated from the sun in the same time.

The rate of shrinkage corresponding to the present rate of solar radiation has been proved to us, by the consideration of our dynamical model, to be 35 metres on the radius per year, or one ten-thousandth of its own length on the radius per two thousand years. Hence, if the solar radiation has been about the same as at present for two hundred thousand years, his radius must have been greater by one per cent. two hundred thousand years ago than at present. If we wish to carry our calculations much farther back or forward than two hundred thousand years, we must reckon by differences of the reciprocal of the sun's radius, and not by differences simply of the radius, to take into account the change of density (which, for example, would be three per cent. for one per cent. change of the radius). Thus the rule, easily worked out according to the principles illustrated by our mechanical model, is this:—

Equal differences of the reciprocal of the radius correspond to equal quantities of heat radiated away from million of years to million of years.

Take two examples—

(1) If in past time there has been as much as fifteen million times the heat radiated from the sun as is at present radiated out in one year, the solar radius must have been four times as great as at present.

(2) If the sun's effective thermal capacity can be maintained by shrinkage till twenty million times the present year's amount of heat is radiated away, the sun's radius must be half what it is now. But it is to be remarked that the density which this would imply, being 11·2 times the density of water, or just about the density of lead, is probably too great to allow the free shrinkage as of a cooling gas to be still continued without obstruction through overcrowding of the molecules. It seems, therefore, most probable that we cannot for the future reckon on more of solar radiation than, if so much as, twenty million times the amount at present radiated out in a year. It is also to be remarked that the greatly diminished radiating surface, at a much lower temperature, would give out annually much less heat than the sun in his present condition gives. The same considerations led Newcomb to the conclusion "that it is hardly likely that the sun can continue to give sufficient heat to support life on the earth (such life as we now are acquainted with, at least) for ten million years from the present time."

In all our calculations hitherto we have for simplicity taken the density as uniform throughout, and equal to the true mean

density of the sun, being about 1·4 times the density of water, or about a quarter of the earth's mean density. In reality the density in the upper parts of the sun's mass must be something less than this, and something considerably more than this in the central parts, because of the pressure in the interior increasing to something enormously great at the centre. If we knew the distribution of interior density we could easily modify our calculations accordingly; but it does not seem probable that the correction could, with any probable assumption as to the greatness of the density throughout a considerable proportion of the sun's interior, add more than a few million years to the past of solar heat, and what could be added to the past must be taken from the future.

In our calculations we have taken Pouillet's number for the total activity of solar radiation, which practically agrees with Herschel's. Forbes * showed the necessity for correcting the mode of allowing for atmospheric absorption used by his two predecessors in estimating the total amount of solar radiation, and he was thus led to a number 1·6 times theirs. Forty years later Langley,† in an excellently worked out consideration of the whole question of absorption by our atmosphere, of radiant heat of all wave-lengths, accepts and confirms Forbes's reasoning, and by fresh observations in very favourable circumstances on Mount Whitney, 15,000 feet above the sea-level, finds a number a little greater still than Forbes (1·7, instead of Forbes' 1·6, times Pouillet's number). Thus Langley's measurement of solar radiation corresponds to 133,000 horse-power per square metre, instead of the 78,000 horse-power which we have taken, and diminishes each of our times in the ratio of 1 to 1·7. Thus, instead of Helmholtz's twenty million years, which was founded on Pouillet's estimate, we have only twelve millions, and similarly with all our other time reckonings based on Pouillet's results. In the circumstances, and taking fully into account all possibilities of greater density in the sun's interior, and of greater or less activity of radiation in past ages, it would, I think, be exceedingly rash to assume as probable anything more than twenty million years of the sun's light in the past history of the earth, or to reckon on more than five or six million years of sunlight for time to come.

We have seen that the sun draws on no external source for the heat he radiates out from year to year, and that the whole energy of this heat is due to the mutual attraction between his parts acting in conformity with the Newtonian law of gravitation. We have seen how an ideal mechanism, easily imagined and understood, though infinitely far from possibility of realisation, could direct the work done by mutual gravitation between all the parts of the shrinking mass, to actually generate its heat-equivalent in an ocean of white-hot liquid covering the sun's surface, and so keep it white-hot while

* 'Edin. New Phil. Journal,' vol. xxxvi. 1844.

† 'American Journal of Science,' vol. xxvi. March, 1883.

constantly radiating out heat at the actual rate of the sun's heat-giving activity. Let us now consider a little more in detail the real forces and movements actually concerned in the process of cooling by radiation from the uttermost region of the sun, the falling inwards of the fluid thus cooled, the consequent mixing up of the whole mass of the sun, the resulting diminished elastic resistance to pressure in equi-dense parts, and the consequent shrinkage of the whole mass under the influence of mutual gravitation. I must first explain that this "elastic resistance to pressure" is due to heat, and is, in fact, what I have, in the present lecture, called "Sir Humphry Davy's repulsive motion" (p. 5). I called it so because Davy first used the expression "repulsive motion" to describe the fine intermolecular motions to which he and other founders of the Kinetic Theory of Heat attributed the elastic resistance to compression presented by gases and fluids.

Imagine, instead of the atoms and molecules of the various substances which constitute the sun's mass, a vast number of elastic globes, like schoolboys' marbles or billiard balls. Consider first, anywhere on our earth a few million such balls put into a room, large enough to hold a thousand times their number, with perfectly hard walls and ceiling, but with a real wooden floor; or, what would be still more convenient for our purpose, a floor of thin elastic sheet steel, supported by joists close enough together to prevent it from drooping inconveniently in any part. Suppose in the beginning the marbles to be lying motionless on the floor. In this condition they represent the atoms of a gas, as for instance, oxygen, nitrogen, or hydrogen, absolutely deprived of heat, and therefore lying frozen, or as molecular dust strewn on the floor of the containing vessel.

If now a lamp be applied below the oxygen, nitrogen, or hydrogen, the substance becoming warmed by heat conducted through the floor, will rise from its condition of absolutely cold solid, or of incoherent molecular dust, and will spread as a gas through the whole enclosed space. If more and more heat be applied by the lamp the pressure of the gas outwards in all directions against the inside of the enclosing vessel will become greater and greater.

As a rude mechanical analogue to this warming of a gas by heat conducted through the floor of its containing vessel, from a lamp held below it, return to our room with floor strewn with marbles, and employ workmen to go below the floor and strike its underside in a great many places vehemently with mallets. The marbles in immediate contact with the floor will begin to jump from it and fall sharply back again (like water in a pot on a fire simmering before it boils). If the workmen work energetically enough there will be more and more of commotion in the heap, till every one of the balls gets into a state of irregular vibration, up and down, or obliquely, or horizontally, but in no fixed direction; and by mutual jostling the heap swells up till the ceiling of the room prevents it from swelling any further. Suppose now the floor to become, like the walls and

ceiling, absolutely rigid. The workmen may cease their work of hammering, which would now be no more availing to augment the motions of the marbles within, than would be a lamp applied outside to warm the contents of a vessel, if the vessel were made of ideal matter impermeable to heat. The marbles being perfectly elastic will continue for ever * flying about in their room striking the walls and floor and ceiling and one another, and remaining in a constant average condition of denser crowd just over the floor and less and less dense up to the ceiling.

In this constant average condition the average velocity of the marbles will be the same all through the crowd, from ceiling to floor, and will be the same in all directions, horizontal, or vertical, or inclined. The continually repeated blows upon any part of the walls or ceiling will in the aggregate be equivalent to a continuous pressure which will be in simple proportion to the average density of the crowd at the place. The diminution of pressure and density from the floor upwards will be precisely the same as that of the density and pressure of our atmosphere calculated on the supposition of equal temperature at all heights, according to the well-known formula and tables for finding heights by the barometer.

In reality the temperature of the atmosphere is not uniform from the ground upwards, but diminishes at the rate of about 1° C. for every 162 metres of vertical ascent in free air, undisturbed by mountains, according to observations made in balloons by the late Mr. Welsh, of Kew, through a large range of heights. This diminution of temperature upwards in our terrestrial atmosphere is most important and suggestive in respect to the constitution of the solar atmosphere, and not merely of the atmosphere or outer shell of the sun, but of the whole interior fluid mass with which it is continuous. The two cases have so much in common that there is in each case loss of heat from the outer parts of the atmosphere by radiation into space, and that in consequence circulating currents are produced through the continuous fluid, by which a thorough mixing up and down is constantly performed. In the case of the terrestrial atmosphere the lowest parts receive by contact heat from the solid earth, warmed daily by the sun's radiation. On the average of night and day, as the air does not become warmer on the whole, it must radiate out into

* To justify this statement I must warn the reader that the ideal perfectly elastic balls which we are imagining must be supposed somehow to have such a structure that each takes only a definite average proportion of its share of the kinetic energy of the whole multitude, so that on the average there is a constant proportion of energy in the translatory motions of the balls ; the other part being the vibratory or rotational motions of the parts of each ball. For simplicity also we suppose the balls to be perfectly smooth and frictionless, so that we shall not be troubled by need to consider them as having any rotatory motions, such as real balls with real frictional collisions would acquire. The ratio of the two kinds of energy for ordinary gases, according to Clausius, to whom is due this essential contribution to the kinetic theory, is—of the whole energy, three-fifths translational to two-fifths vibrational.

space as much heat as all that it gets, both from the earth by contact, and by radiation of heat from the earth, and by intercepted radiation from the sun on its way to the earth. In the case of the sun the heat radiated from the outer parts of the atmosphere is wholly derived from the interior. In both cases the whole fluid mass is kept thoroughly mixed by currents of cooled fluid coming down and warmer fluid rising to take its place, and to be cooled and descend in its turn.

Now it is a well-known property of gases and of fluids generally (except some special cases, as that of water within a few degrees of its freezing temperature, in which the fluid under constant pressure contracts with rise of temperature) that condensation and rarefactions, effected by augmentations and diminutions of pressure from without, produce elevations and lowerings of temperature in circumstances in which the gas is prevented from either taking heat from or giving heat to any material external to it. Thus a quantity of air or other gas taken at ordinary temperature (say 15° C. or 59° F.) and expanded to double its bulk becomes 71° C. cooler; and if the expansion is continued to thirty-two times its original bulk it becomes cooled 148° farther, or down to about 200° C. below the temperature of freezing water, or to within 73° of absolute cold. Such changes as these actually take place in masses of air rising in the atmosphere to heights of eight or nine kilometers, or of twenty or twenty-five kilometers. Corresponding differences of temperature there certainly are throughout the fluid mass of the sun, but of very different magnitudes because of the twenty-seven fold greater gravity at the sun's surface, the vastness of the space through which there is free circulation of fluid, and last, though not least, the enormously higher temperature of the solar fluid than of the terrestrial atmosphere at points of equal density in the two. This view of the solar constitution has been treated mathematically with great power by Mr. J. Homer Lane, of Washington, U.S., in a very important paper read before the National Academy of Sciences, of the United States in April 1869, and published with further developments in the 'American Journal of Science,' for July 1870. Mr. Lane, by strict mathematical treatment finds the law of distribution of density and temperature all through a globe of homogeneous gas left to itself in space, and losing heat by radiation outwards so slowly that the heat-carrying currents produce but little disturbance from the globular form.

One very remarkable and important result which he finds is, that the density at the centre is about twenty * times the mean density; and this, whether the mass be large or small, and whether of oxygen, nitrogen, or hydrogen, or other substance; provided only it be of one kind of gas throughout, and that the density in the central parts is not too great to allow the condensation to take place, according to

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* Working out Lane's problem independently, I find 22½ as very nearly the exact number.

the ordinary gaseous law of density, in simple proportion to pressure for the same temperatures. We know this law to hold with somewhat close accuracy for common air, and for each of its two chief constituents, oxygen and nitrogen, separately, and for hydrogen, to densities of about two hundred times their densities at our ordinary atmospheric pressure. But when the compressing force is sufficiently increased, they all show greater resistance to condensation than according to the law of simple proportion, and it seems most probable that there is for every gas a limit beyond which the density cannot be increased by any pressure however great. Lane remarks that the density at the centre of the sun would be "nearly one-third greater than that of the metal platinum," if the gaseous law held up to so great a degree of condensation for the ingredients of the sun's mass; but he does not suggest this supposition as probable, and he no doubt agrees with the general opinion that in all probability the ingredients of the sun's mass, at the actual temperatures corresponding to their positions in his interior, obey the simple gaseous law through but a comparatively small space inwards from the surface; and that in the central regions they are much less condensed than according to that law. According to the simple gaseous law, the sun's central density would be thirty-one times that of water; we may assume that it is in all probability much less than this, though considerably greater than the mean density, 1·4. This is a wide range of uncertainty, but it would be unwise at present to narrow it, ignorant as we are of the main ingredients of the sun's whole mass, and of the laws of pressure, density, and temperature, even for known kinds of matter at very great pressures and very high temperatures.

The question, Is the sun becoming colder or hotter? is an exceedingly complicated one, and, in fact, either to put it or to answer it is a paradox, unless we define exactly where the temperature is to be reckoned. If we ask, How does the temperature of equi-dense portions of the sun vary from age to age? the answer certainly is that the matter of the sun of which the density has any stated value, for example, the ordinary density of our atmosphere, becomes always less and less hot, whatever be its place in the fluid, and whatever be the law of compression of the fluid, whether the simple gaseous law or anything from that to absolute incompressibility. But the distance inwards from the surface at which a constant density is to be found diminishes with shrinkage, and thus it may be that at constant depths inwards from the bounding surface the temperature is becoming higher and higher. This would certainly be the case if the gaseous law of condensation held throughout, but even then the effective radiational temperature, in virtue of which the sun sheds his heat outwards, might be becoming lower, because the temperatures of equi-dense portions are clearly becoming lower under all circumstances.

Leaving now these complicated and difficult questions to the

scientific investigators who are devoting themselves to advancing the science of solar physics, consider the easily understood question, What is the temperature of the centre of the sun at any time, and does it rise or fall as time advances? If we go back a few million years to a time when we may believe the sun to have been wholly gaseous to the centre, then certainly the central temperature must have been augmenting; again, if, as is possible though not probable at the present time, but may probably be the case at some future time, there be a solid nucleus, then certainly the central temperature would be augmenting, because the conduction of heat outwards through the solid would be too slow to compensate the augmentation of pressure due to augmentation of gravity in the shrinking fluid around the solid. But at a certain time in the history of a wholly fluid globe, primitively rare enough throughout to be gaseous, shrinking under the influence of its own gravitation and its radiation of heat outwards into cold surrounding space, when the central parts have become so much condensed as to resist further condensation greatly more than according to the gaseous law of simple proportions, it seems to me certain that the early process of becoming warmer, which has been demonstrated by Lane, and Newcomb, and Ball, must cease, and that the central temperature must begin to diminish on account of the cooling by radiation from the surface, and the mixing of the cooled fluid throughout the interior.

Now we come to the most interesting part of our subject—the early history of the Sun. Five or ten million years ago he may have been about double his present diameter and an eighth of his present mean density, or ·175 of the density of water; but we cannot, with any probability of argument or speculation, go on continuously much beyond that. We cannot, however, help asking the question, What was the condition of the sun's matter before it came together and became hot? It may have been two cool solid masses, which collided with the velocity due to their mutual gravitation; or, but with enormously less of probability, it may have been two masses colliding with velocities considerably greater than the velocities due to mutual gravitation. This last supposition implies that, calling the two bodies A and B for brevity, the motion of the centre of inertia of B relatively to A, must, when the distances between them was great, have been directed with great exactness to pass through the centre of inertia of A; such great exactness that the rotational momentum, or "moment of momentum," * after collision was no more than to let

* This is a technical expression in dynamics which means the importance of motion relatively to revolution or rotation round an axis. Momentum is an expression given about a hundred and fifty years ago (when mathematicians and other learned men spoke and wrote Latin) to signify translational importance of motion. Moment of a couple, moment of a magnet, moment of inertia, moment of force round an axis, moment of momentum round an axis, and corresponding verbal combinations in French and German, are expressions which have been

the sun have his present slow rotation when shrunk to his present dimensions. This exceedingly exact aiming of the one body at the other, so to speak, is, on the dry theory of probability, exceedingly improbable. On the other hand, there is certainty that the two bodies A and B at rest in space if left to themselves undisturbed by other bodies and only influenced by their mutual gravitation, shall collide with direct impact, and therefore with no notion of their centre of inertia, and no rotational momentum of the compound body after the collision. Thus we see that the dry probability of collision between two neighbours of a vast number of mutually attracting bodies widely scattered through space is much greater if the bodies be all given a rest, than if they be given moving in any random directions and with any velocities considerable in comparison with the velocities which they would acquire in falling from rest into collision. In this connection it is most interesting to know from stellar astronomy, aided so splendidly as it has recently been by the spectroscope, that the relative motions of the visible stars and our sun are generally very small in comparison with the velocity (612 kilometers per second) which a body would acquire in falling into the sun, and are comparable with the moderate little velocity (29·5 kilometres per second) of the earth in her orbit round the sun.

To fix the ideas, think of two cool solid globes, each of the same mean density as the earth, and of half the sun's diameter; given at rest, or nearly at rest, at a distance asunder equal to twice the earth's distance from the sun. They will fall together and collide in exactly half a year. The collision will last for about half an hour, in the course of which they will be transformed into a violently agitated incandescent fluid mass flying outward from the line of the motion before the collision, and swelling to a bulk several times greater than the sum of the original bulks of the two globes.* How far the fluid mass will fly out all round from the line of collision it is impossible to say. The motion is too complicated to be fully investigated by any known mathematical method; but with sufficient patience a mathematician might be able to calculate it with some fair approximation to the truth. The distance reached by the extreme circular

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introduced within the last sixty years (by scientists speaking as now, each his own vernacular) to signify the importance of the special subject referred to in each case. The expression moment of momentum is highly valuable and convenient in dynamical science, and it constitutes a curious philological monument of scientific history.

* Such incidents seem to happen occasionally in the universe. Laplace says some stars "have suddenly appeared, and then disappeared, after having shone for several months with the most brilliant splendour. Such was the star observed by Tycho Brahe in the year 1572, in the constellation Cassiopeia. In a short time it surpassed the most brilliant stars, and even Jupiter itself. Its light then waned away, and finally disappeared sixteen months after its discovery. Its colour underwent several changes; it was at first of a brilliant white, then of a reddish yellow, and finally of a lead-coloured white, like to Saturn." (Harte's translation of Laplace's 'System of the World.' Dublin, 1830.)

fringe of the fluid mass would probably be much less than the distance fallen by each globe before the collision, because the translational motion of the molecules constituting the heat into which the whole energy of the original fall of the globes become transformed in the first collision, is probably about three-fifths of the whole amount of that energy. The time of flying out would probably be less than half a year, when the fluid mass must begin to fall in again towards the axis. In something less than a year after the first collision the fluid will again be in a state of maximum crowding round the centre, and this time probably even more violently agitated than it was immediately after the first collision; and it will again fly outward, but this time axially towards the places whence the two globes fell. It will again fall inwards, and after a rapidly subsiding series of quicker and quicker oscillations it will subside, probably in the course of two or three years, into a globular star of about the same dimensions, heat, and brightness as our present sun, but differing from him in this, that it will have no rotation.

We supposed the two globes to have been at rest when they were let fall from a mutual distance equal to the diameter of the earth's orbit. Suppose, now, that instead of having been at rest they had been moving in opposite directions with a velocity of two (more exactly 1·89) metres per second. The moment of momentum of these motions round an axis through the centre of gravity of the two globes perpendicular to their lines of motion is just equal to the moment of momentum of the sun's rotation round his axis. It is an elementary and easily proved law of dynamics that no mutual action between parts of a group of bodies, or of a single body, rigid, flexible, or fluid, can alter the moment of momentum of the whole. The transverse velocity in the case we are now supposing is so small that none of the main features of the collision and of the wild oscillations following it, which we have been considering, or of the magnitude, heat, and brightness of the resulting star, will be sensibly altered; but now, instead of being rotationless, it will be revolving once round in twenty-five days and so in all respects like to our sun.

If instead of being at rest initially, or moving with the small transverse velocities we have been considering, each globe had a transverse velocity of three-quarters (or anything more than ·71) of a kilometre per second, they would just escape collision, and would revolve in ellipses round the centre of inertia in a period of one year, just grazing each other's surface every time they came to the nearest points of their orbits.

If the initial transverse velocity of each globe be less than, but not much less than, ·71 of a kilometre per second, there will be a violent grazing collision, and two bright suns, solid globes bathed in flaming fluid, will come into existence in the course of a few hours, and will commence revolving round their common centre of inertia in long elliptic orbits in a period of a little less than a year. Tidal interaction between them will diminish the eccentricities of their

orbits, and if continued long enough will cause the two to revolve in circular orbits round their centre of inertia with a distance between their surfaces equal to 6·44 diameters of each.

Suppose now, still choosing a particular case to fix the ideas, that twenty-nine million cold solid globes, each of about the same mass as the moon, and amounting in all to a total mass equal to the sun's, are scattered as uniformly as possible on a spherical surface of radius equal to one hundred times the radius of the earth's orbit, and that they are left absolutely at rest in that position. They will all commence falling towards the centre of the sphere, and will meet there in two hundred and fifty years, and every one of the twenty-nine million globes will then, in the course of half an hour, be melted, and raised to a temperature of a few hundred thousand or a million degrees Centigrade. The fluid mass thus formed will, by this prodigious heat, be exploded outwards in vapour or gas all round. Its boundary will reach to a distance considerably less than one hundred times the radius of the earth's orbit on first flying out to its extreme limit. A diminishing series of out and in oscillations will follow, and the incandescent globe thus contracting and expanding alternately, in the course it may be of three or four hundred years, will settle to a radius of forty* times the radius of the earth's orbit. The average density of the gaseous nebula thus formed would be $(215 \times 40)^{-3}$, or one six hundred and thirty-six thousand millionth of the sun's mean density; or one four hundred and fifty-four thousand millionth of the density of water; or one five hundred and seventy millionth of that of common air at an ordinary temperature of 10° C. The density in its central regions, sensibly uniform through several million kilometres, is (see note on p. 11) one twenty thousand millionth of that of water; or one twenty-five millionth of that of air. This exceedingly small density is nearly six times the density of the oxygen and nitrogen left in some of the receivers exhausted by Bottomley in his experimental measurements of the amount of heat emitted by pure radiation from highly heated bodies. If the substance were oxygen, or nitrogen, or other gas or mixture of gases simple or compound, of specific density equal to the specific density of our air, the central temperature would be 51,200° Cent., and the average translational velocity of the molecules 6·66 kilometres per second, being $\sqrt{\frac{3}{7}}$ of 10·2, the velocity acquired by a heavy body falling unresisted from the outer boundary (of 40 times the radius of the earth's orbit) to the centre of the nebulous mass.

The gaseous nebula thus constituted would in the course of a few million years, by constantly radiating out heat, shrink to the size of our present sun, when it would have exactly the same heating and lighting efficiency. But no motion of rotation.

* The radius of a steady globular gaseous nebula of any homogeneous gas is 40 per cent. of the radius of the spherical surface from which its ingredients must fall to their actual positions in the nebula to have the same kinetic energy as the nebula has.

The moment of momentum of the whole solar system is about eighteen times that of the sun's rotation; seventeen-eighteenths being Jupiter's and one-eighteenth the Sun's, the other bodies being not worth taking into account in the reckoning of moment of momentum.

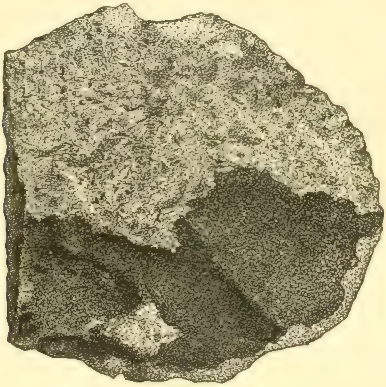
Now instead of being absolutely at rest in the beginning, let the twenty-nine million moons be given each with some small motion, making up in all an amount of moment of momentum about a certain axis, equal to the moment of momentum of the solar system which we have just been considering; or considerably greater than this, to allow for effect of resisting medium. They will fall together for two hundred and fifty years, and though not meeting precisely in the centre as in the first supposed case of no primitive motion, they will, two hundred and fifty years from the beginning, be so crowded together that there will be myriads of collisions, and almost every one of the twenty-nine million globes will be melted and driven into vapour by the heat of these collisions. The vapour or gas thus generated will fly outwards, and after several hundreds or thousands of years of outward and inward oscillatory motion, may settle into an oblate rotating nebula extending its equatorial radius far beyond the orbit of Neptune, and with moment of momentum equal to or exceeding the moment of momentum of the solar system. This is just the beginning postulated by Laplace for his nebular theory of the evolution of the solar system; which, founded on the natural history of the stellar universe, as observed by the elder Herschell, and completed in details by the profound dynamical judgment and imaginative genius of Laplace, seems converted by thermodynamics into a necessary truth, if we make no other uncertain assumption than that the materials at present constituting the dead matter of the solar system have existed under the laws of dead matter for a hundred million years. Thus there may in reality be nothing more of mystery or of difficulty in the automatic progress of the solar system from cold matter diffused through space, to its present manifest order and beauty, lighted and warmed by its brilliant sun, than there is in the winding up of a clock* and letting it go till it stops. I need scarcely say that the beginning and the maintenance of life on the earth is absolutely and infinitely beyond the range of all sound speculation in dynamical science. The only contribution of dynamics to theoretical biology is absolute negation of automatic commencement or automatic maintenance of life.

I shall only say in conclusion:—Assuming the sun's mass to be composed of materials which were far asunder before it was hot, the immediate antecedent to its incandescence must have been either two bodies with details differing only in proportions and densities from

* Even in this, and all the properties of matter which it involves, there is enough, and more than enough, of mystery to our limited understanding. A watch-spring is much farther beyond our understanding than is a gaseous nebula.

the cases we have been now considering as examples; or it must have been some number more than two—some finite number—at the most the number of atoms in the sun's present mass, a finite number (which may probably enough be something between 4×10^{57} and 140×10^{57}) as easily understood and imagined as number 4 or 140. The immediate antecedent to incandescence may have been the whole constituents in the extreme condition of subdivision—that is to say, in the condition of separate atoms; or it may have been any smaller number of groups of atoms making minute crystals or groups of crystals—snowflakes of matter, as it were; or it may have been lumps of matter like a macadamising stone; or like this stone* (Fig. 1), which you might mistake for a macadamising

Fig. 1.



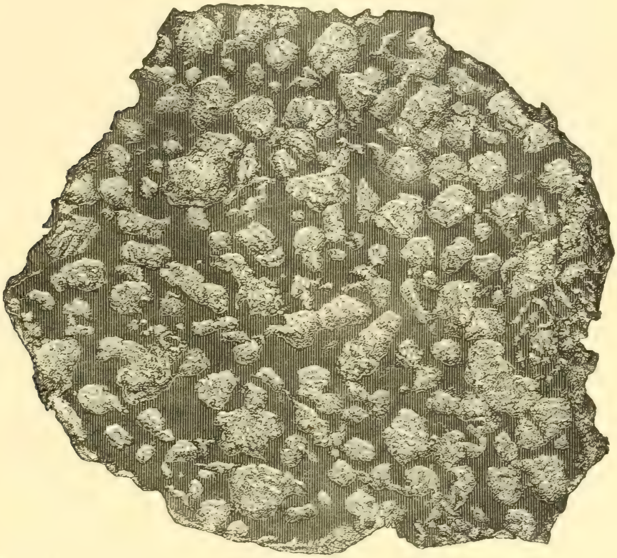
←.........5 centimetres.........→

stone, and which was actually travelling through space till it fell on the earth at Possil, in the neighbourhood of Glasgow, on April 5, 1804; or like that* (Fig. 2) which was found in the Desert of Atacama, in South America, and is believed to have fallen there from the sky—a fragment made up of iron and stone, which

* These three meteorites are in the possession of the Hunterian Museum of the University of Glasgow, and the woodcuts, Figs. 1, 2, and 3, have been executed from the actual specimens kindly lent for that purpose by the keeper of the museum, Professor Young. The specimen represented by Fig. 1 is contained in the Hunterian collection, that by Fig. 2 in the Eck collection, and that by Fig. 3 in the Lanfine collection—the scale of dimensions is shown for each. It may be remarked that Fig. 2 represents a section of the meteorite taken in the plane of the longest rectangular axes; the bright markings being large and well-formed crystals of olivine, embedded in a matrix of iron. In Fig. 3 is depicted the beautiful Widmanstätten marking characteristic of all meteoric iron, and so well shown in the well-known Lenarto meteorite.

looks as if it has solidified from a mixture of gravel and melted iron in a place where there was very little of heaviness; or this splendidly crystallised piece of iron (Fig. 3), a slab cut out

FIG. 2.



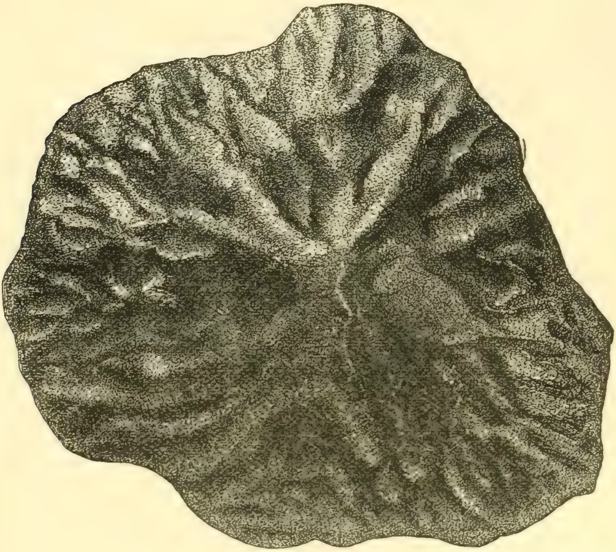
←....................$13\frac{1}{2}$ centimetres....................→

FIG. 3.

←..................$9\frac{1}{4}$ centimetres............ ....→

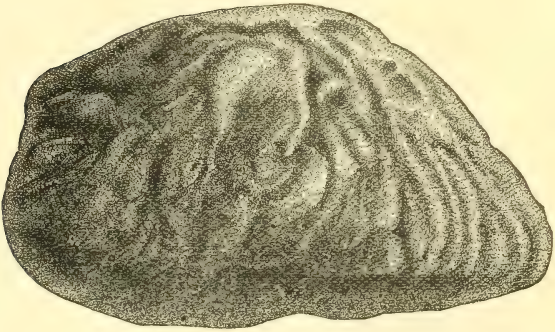
of the celebrated aërolite which fell at Lenarto, in Hungary; * or this wonderfully-shaped specimen (of which two views are given

FIG. 4.



←.....................15 centimetres.....................→

FIG. 5.



in Figs. 4 and 5), a model of the Middlesburgh meteorite (kindly given me by Professor A. S. Herschel), having corrugations showing

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* See footnote, p. 18.

how its melted matter has been scoured off from the front part of its surface in its final rush through the earth's atmosphere when it was seen to fall on March 14, 1881, at 3.35 P.M.

For the theory of the sun it is indifferent which of these varieties of configurations of matter may have been the immediate antecedent of his incandescence, but I can never think of these material antecedents without remembering a question put to me thirty years ago by the late Bishop Ewing, Bishop of Argyll and the Isles: "Do you imagine that piece of matter to have been as it is from the beginning; to have been created as it is, or to have been as it is through all time till it fell on the earth?" I had told him that I believed the sun to be built up of meteoric stones, but he would not be satisfied till he knew or could imagine, what kind of stones. I could not but agree with him in feeling it impossible to imagine that any one of such meteorites as those now before you has been as it is through all time, or that the materials of the sun were like this for all time before they came together and became hot. Surely this stone has an eventful history, but I shall not tax your patience by trying just now to trace it conjecturally. I shall only say that we cannot but agree with the common opinion which regards meteorites as fragments broken from larger masses, and we cannot be satisfied without trying to imagine what were the antecedents of those masses.

[W. T.]

WEEKLY EVENING MEETING,

Friday, January 28, 1887.

Sir John Lubbock, Bart. M.P. D.C.L. LL.D. F.R.S.
Vice-President, in the Chair.

Professor W. Baldwin Spencer.

The Pineal Eye in Lizards.

There is present in the human brain, buried deeply beneath the surface and hence concealed from view externally, a small blunt process: it differs in nature from the surrounding nervous matter, being hard to the touch. To this small process human anatomists gave the name of *pineal gland*, and its meaning has always remained a problem. Investigation of the brains of other animals showed that in these the structure was present: in fact it is typical of the brains of all true vertebrata, and not only this, the lower we descend in the scale of vertebrate life, the more highly developed does it become in comparison to the remaining parts of the brain. In such a mammal as a rabbit it is, for example, larger than in man; in a bird it is still more highly developed; whilst when we come down to fishes, the pineal gland or *epiphysis*, as it is better called, assumes the form of a forwardly directed hollow process whose distal extremity is swollen out into a vesicle, the proximal part forming a hollow stalk running back to the roof of the brain. If the brain of a lizard, such as *Hatteria*, be examined, the epiphysis is seen to have undergone a very definite change: it is divided more clearly than in other animals into the two divisions above mentioned; sections, however, show that the stalk is solid and that the vesicle has become developed into an organ of vision—into a *pineal eye*. This is the highest stage of development reached by the epiphysis, and is now preserved, so far as is at present known, in lizards only amongst living animals.

Turning to the development of the brain: it is seen in early stages in all vertebrates to have the form of a simple tube running the whole length of the back of the animal; a little later the walls of the anterior division have bulged out and given rise to three vesicles which are known as the fore, mid, and hind brains; the part of the tube posterior to these forms the spinal cord. As development proceeds, from the fore brain on either side is given off a hollow process, each one of which forms an optic vesicle, and at the same time there grow forward two outgrowths of the fore brain which give rise to the cerebral hemispheres; in addition to these structures, the roof of the fore brain gives off a single median outgrowth which grows forward within the skull cavity, swelling out distally into a small

vesicle: in the skull wall itself in most lizards there is present a distinctly marked hole, and in this, which is called the parietal foramen, lies the small vesicle. Changes take place in the walls of the optic vesicles which transform them into the sensory parts of the paired eyes, whilst the small vesicle formed out of the dorsal outgrowth of the brain, or epiphysis, becomes formed into the single *pineal eye.* Thus from the walls of the fore brain are developed in lizards three eyes—a single median and two paired ones.

Digressing for a moment, it is interesting to notice that this is by no means the only example of a single median eye which is known in the animal kingdom. Almost every people has its myth of a median-eyed race of men, who may, as in the Grecian Cyclops, be represented with the single eye, or with this in addition to the paired eyes: it is curious also to find that Buddha and Siva are represented with median eyes: whilst to such an extent is this idea carried, that even the Nautch girl paints on her forehead the outline of a single median eye.

Leaving the mythical we come to animals in which such a structure is actually present: in the class Crustacea we meet with many examples of this; first in the little fresh-water Cyclops and all its allies, there is only the one solitary eye, the presence of which has gained for it its name. In other Crustacea again we find that the animal on leaving the egg-case has a very definite form, quite unlike that of the adult: it is, amongst other things, always provided with three, and no more or less than three pairs of appendages by means of which it moves about; it has in addition always a single median eye which, inasmuch as the animal in this stage is called a nauplius, is known as the nauplius eye. As development goes on, there appear two eyes, one at either side of the original one: in some cases the latter may disappear, in others it may be retained, though the lateral paired eyes always become larger than the single one, and thus, though there is no real connection between the two whatever, we find in Crustacea a median and two paired eyes present just as in lizards.

Turning to the latter, we find the epiphysis, as before said, more highly developed than in any other living animal. The distal vesicular expansion is present in many forms, but here only is it transformed into an eye and connected with the brain by a solid pineal stalk developed from the proximal part of the epiphysis and serving doubtless as an optic nerve.

If the head of a lizard, such as an *Iguana* or *Calotes*, be examined, there is seen on the middle line dorsally, and somewhat behind the level of the paired eyes, a peculiarly modified scale; it bears a circular space, which may be raised into a dome-shape, or may be merely surrounded by a raised rim, but is always noticeable by its whiteness, this being due to an absence of pigment in the skin of this particular spot. The modified scale indicates the position of the pineal eye lying beneath, and thus of the parietal foramen enclosing the eye: it does not necessarily follow that no eye is present, because

there is no special external indication, nor, on the other hand, does the presence of the latter indicate infallibly a well-developed eye.

Thus in *Hatteria*, the New Zealand lizard, there is no external indication of the eye, save perhaps a slight absence of pigment in the skin immediately above the parietal foramen; but if longitudinal vertical sections be cut through the head, the eye is found lying deeply embedded in connective tissue within the foramen. It has the form somewhat of a cone, whose base is directed forwards and upwards, whilst the apex points backwards and is united with the pineal stalk. The base of the cone is formed by the lens, which consists of elongate, nucleated cells, arranged so as to form a cone whose apex points inwards and lies in the line of the optic axis; the lens is directly continuous with the retina, and thus, unlike that of the paired eyes, is formed directly out of what was originally part of the brain-wall. The retina itself is well developed in *Hatteria*, and forms a strong contrast to that of the lateral eyes, inasmuch as the rods, embedded in dark-brown pigment, face inwards and line the cavity of the vesicle, whilst in the paired eye, the same structures are on the side of the retina remote from the cavity of the eye in the adult, though this, it must be remembered, is not homologous with the cavity in the pineal eye. External to the rods lies first a layer of spherical, nucleated elements, then a molecular layer consisting of finely punctated material, then another layer of spherical elements, followed by a layer of cone and spindle-shaped structures, which again lie directly upon a thin layer of nerve-fibres spreading out from the pineal stalk. The rods lying in the optic axis are much elongated and very prominent, a feature common to all eyes which are still in connection with the optic stalk. This may be taken as a description of the retina of a typical pineal eye, though it is more highly developed in *Hatteria* than in many other lizards at the present day. The remainder of the epiphysis may be divided into two parts, a solid pineal stalk nearest the eye, and a hollow part running back to the roof of the brain and overlying, as it nears the latter, the vascular roof of the third ventricle.

In many lizards degeneration seems to have set in and the eye to have become more rudimentary than it is in *Hatteria*; in fact, in the latter, the only sign of its rudimentary nature is its position deeply buried in the connective tissue within the parietal foramen, a position which must prevent its functioning as an organ of vision at the present time. If we take such a lizard as *Varanus bengalensis*, sections through the parietal foramen show that the eye is well developed and placed not far below the surface of the head, whilst between it and the latter is a marked absence of the pigment cells which form so prominent a feature in sections of the skin all around. Further examination however reveals an important point of difference when compared with *Hatteria*—*the eye has lost its connection with the proximal part of the epiphysis*, which has the form of a hollow process

running forwards from the roof of the brain within the skull till it reaches the parietal foramen; but the distal vesicle, transformed into an eye, has become completely separated off from it. This again may be taken as an example of a large number of lizards, including such forms as *Varanus bengalensis*, *Anguis fragilis*, *Seps chalcidica* and *Calotes ophiomaca*. In other forms, still further degeneration seems to have taken place, or, to speak more correctly, the epiphysis appears to develop to a certain point, but never, at the present time, to reach the stage at which it becomes transformed into an eye. If the brain of *Cyclodus gigas* be examined, the epiphysis when cut in section has merely the form of a hollow dorsal process running forward from the brain till it reaches the parietal foramen; within this it expands to form a vesicle, but apparently development stops at this, which must necessarily be a stage passed through in the formation of every pineal eye. The pineal stalk never becomes solid, nor do the walls of the vesicular enlargement become modified into lens and retina, though there is a slight difference to be noticed between the anterior and posterior walls, indicating perhaps the earliest of the series of changes, whereby out of the former is produced the lens, and out of the latter the rods and external-lying elements of the retina.

These three examples—*Hatteria*, *Varanus*, and *Cyclodus*—will serve to illustrate the more important stages of development met with at the present time in the epiphysis of lizards.

We may now turn for a minute or two to the consideration of the question, whether it is possible to connect this single median eye with any structure in lower forms, whether the latter, in fact, possess any organ out of which we may suppose the epiphysis of higher forms to have been gradually evolved. Amongst the lowest forms of Vertebrata known to us—the Tunicata—we find that at one stage of their development they possess a dorsal nerve-cord, whose anterior end becomes swollen out and thus forms what we may call a brain; in fact this homology with the same part in higher forms is further strengthened by its relationship to the anterior end of the notochord which, running the whole length beneath the posterior part of the nerve-cord, stops just before reaching this anterior swollen extremity. In the adult of most forms of Tunicata the latter is the only persistent portion of the whole nervous system, and its development alone enables us to homologise it with the brain of such an animal as a lizard. Now if this Tunicate brain be examined it is found to possess a single eye placed not quite but nearly in the median line, *but completely within the brain cavity.* The Tunicata are, individually, small and transparent, and thus the light can as easily affect an internal as an externally-placed eye. The question naturally arises, supposing the Tunicata to have developed from some ancestor common to them and the higher Vertebrata, has this single eye anything to do with the pineal eye? The latter cannot be directly developed out of the

former, that is, there must have been a stage passed through in which the internal eye of the Tunicate-like ancestor was undergoing evagination, and during which period it could not function as an organ of vision, but it is possible that the internal eye of the small, transparent form became changed into the externally-placed epiphysis of the larger and more opaque animal which was gradually evolved out of the Tunicate-like ancestor. By further and secondary modification the epiphysis gave rise to a distal vesicle and a proximal pineal stalk, whilst still later again the former became modified into the pineal eye.

Henri de Graaf in his memoir on the development of the epiphysis, mentions as a curious fact in its formation in *Bufo cinerea*, that it originates as a thickening of the brain roof and that there is a small mass of pigment present for some little time at the inner ends of the cells forming the thickening; now this is exactly the development, according to Kowalevsky, which is passed through in early stages during the formation of the eye of a larval Tunicate; in the latter the eye comes subsequently to lie within the brain, still more pigment being developed and a lens formed, whilst in *Bufo* the pigment disappears, the thickening forms into an evagination, and the epiphysis, as present in all higher Vertebrata, is gradually formed. It is quite possible, however, that this may present us with some of the steps passed through in the gradual development of the epiphysis out of the internally placed eye of the Tunicate-like ancestors of living Vertebrata. We may perhaps go even a step further and find in these low vertebrate forms the structures which by a similar evagination have given rise to the optic vesicles out of which by a later modification were developed the paired eyes. In one form of Tunicata—the Salps—are found in the brain (that is the persistent anterior extremity of the larval nerve-cord), not one, but three eyes, of which one is median and two are lateral; we may perhaps be here presented with the organs which have gradually given rise, during the change from a transparent to an opaque animal, to the three vesicles which in all Vertebrata, save the Tunicata and Amphioxus, now form the single dorsal epiphysis and the paired, lateral, optic vesicles.

At the present time the median pineal eye is found only in lizards, and even here it is but in a rudimentary condition; it is always intimately associated with the presence of the parietal foramen; if the foramen be present the eye is found in a more or less highly developed state, if the foramen be wanting the eye is absent. It is curious to note that the foramen is a marked characteristic of extinct forms of vertebrate life—of the Labyrinthodonts of Palæozoic, and the Saurians of Mesozoic times; so far as can be told it is simply formed to allow the distal extremity of the epiphysis to pass through the skull roof, and thus to enable the pineal eye to be placed upon the surface of the head. We are probably right in assuming that the eye was most largely developed in these extinct forms, and thus it is

peculiarly interesting to note that at the present day it is found most highly developed in *Hatteria*, which, sheltered from competition of surrounding forms in its island home in New Zealand, has retained this, amongst other archaic features, pointing to its close alliance with the extinct reptilian fauna of Mesozoic times.

[W. B. S.]

WEEKLY EVENING MEETING,

Friday, February 4, 1887.

Henry Pollock, Esq. Treasurer and Vice-President, in the Chair.

Edwin Freshfield, Esq. LL.D. V.P.S.A.

Some Unpublished Records of the City of London.

In the records of the 113 parishes within the City of London, and the 17 out-parishes, there are materials for a parochial and social history unrivalled in the world.

These records which, commencing with the 15th century, extend to the present time in a more or less complete state, consist of books of records, vestry minutes, and account books and registers. These books contain lists of the parish property, sanitary orders made by the various parishes, their regulations for self-government, and evidences of the great destruction of valuable works of art which took place in King Edward VI.'s reign, at the Reformation. The particular books from which extracts have been selected, are those of St. Margaret's, Lothbury, St. Stephen's, Coleman Street, St. Bartholomew by the Exchange, and St. Christopher-le-Stocks. The minutes of the vestries describe accurately the relations of the clergy to the parishioners during the sixteenth and early part of the seventeenth century, the causes which led to the complete break between the clergy and laity which took place at the commencement of the Great Rebellion in 1642. These also show the working of the sanitary arrangements during the same period, particularly those having reference to the various epidemics of the plague. That which is in some respects the most interesting portion of these books, is the history of the Great Rebellion, the Commonwealth, the Protectorate, and the Restoration. In order to illustrate this, the history of the parish of St. Bartholomew by the Exchange, as displayed in the parish books, has been supplemented by extracts from the books of the adjoining parishes during a corresponding period.

At the outbreak of the Great Rebellion, the rector of St. Bartholomew's—Dr. Grant—was an easy-going Churchman, who had rendered himself liable to sequestration for refusing to sign the "solemn league and covenant." The parishioners, numbering among them Sir Harbottle Grimstone, Mr. Justice Peter Pheasant, and Dr. Zouch, came to an arrangement with him—which is embodied in the vestry minutes—whereby, in exchange for a pension, he surrendered the parsonage house, and the right to officiate in the church during his life, conferring the right of nominating a *locum tenens* upon the parishioners. This right they exercised from the year 1644 until Dr. Grant's death in 1653. During this period, the living was held

first by Dr. Lightfoot (a Presbyterian), the celebrated Rabbinical scholar, afterwards by Mr. Cawton, another Presbyterian, who was imprisoned for praying for King Charles II. after the execution of his father, and finally had to leave the country in consequence of his association with Mr. Love, the rector of St. Ann and St. Agnes, who was executed on Tower Hill for negotiating with the Scotch for bringing King Charles II. to England. During the incumbency of Dr. Lightfoot and Mr. Cawton, the Presbyterian Church government was in full force in the parish, and the nature of it, and the dissatisfaction which it caused to the inhabitants, is clear from the parish books. After Mr. Cawton's flight to Holland, the parishioners appointed another Presbyterian named Mr. Hall, who continued until the death of Dr. Grant. In 1653, the living then being vacant, and in the gift of the Commissioners of the Great Seal, they appointed one Sidrach Simpson, a well-known Independent. He died about eighteen months after his appointment, having in the meantime been suspended and imprisoned by the Protector Oliver, in consequence of his having preached against his personal government. On Mr. Sidrach Simpson's death, the Protector appointed—without consulting the Commissioners of the Great Seal—by a writing under his own hand, to the living one of his chaplains, Mr. Philip Nye, who associated with him another Independent minister named Mr. John Loder. The minutes from this time to the months immediately preceding the Restoration, give an interesting account of the quarrels between Mr. Nye and Mr. Loder and the parishioners, on the ground that they refused to administer the sacrament and to christen children, except the parishioners would be joined in communion with their Church, the parishioners also refusing to pay tithes, on the ground that their church was taken up and their pews filled with strange congregations. Mr. Loder, on behalf of Mr. Nye, offered the parishioners to allow them to choose a minister to officiate in the afternoons of the Lord's Day in a manner which was adopted in the adjoining parish of St. Stephen's, Coleman Street, where Mr. John Godwin, the Independent, shared the church with Mr. John Taylor, the Presbyterian vicar, but this the parishioners of St. Bartholomew's refused.

In January 1659 the Lord General Monk came to London, and on his demand the secluded members were reinstated in the resuscitated Long Parliament, then sitting, and fresh Commissioners of the Great Seal were appointed.

The parishioners took the opportunity to petition the Commissioners to declare the living vacant on the ground that the Protector Oliver had improperly presented to the living which was in their gift. The books show how Mr. Loder had obtained from the Lord General Monk a letter to the Commissioners in his favour, which letter the Lord General at a subsequent request of the parishioners revoked, and how the Commissioners refused to appoint any of the Independent faction and ultimately appointed Dr. Brideoak, one of the defenders of Lathom House and a friend of the Speaker

Lenthall. They illustrate the state of affairs and the Lord General's own condition of mind at this particular juncture, viz. in the months of March and April 1659–60. In March he had not yet broken with the Independent faction and thrown in his lot with the Royalists. It was probably at this time that the first letter in favour of Mr. Loder was written. The letter revoking the first letter was no doubt written at the time when the Lord General had determined to throw in his lot with the Royalists. In a minute of the same vestry in September 1659, the churchwarden mentions an application to the Grocers' Company to take the balance of the parish stock upon interest and the fact that the Grocers' Company would not pay him more than 5 per cent. interest, the minute concluding "that in regard to the existing great hazard and danger of the times by reason of public differences and decay of trade in general, the money aforesaid, viz. 150*l.*, was paid into the hands of the Grocers at 5 per cent."

The lively interest taken in parochial matters by Speakers of the House of Commons, Masters of the Rolls, lawyers, merchants, and people holding high Government appointments, is apparent from these books and compared favourably with the present apathy. They also illustrate the extraordinary personal nature of the Protector's government during the Protectorate, and the important part played by the City of London at this time, the reason why the citizens threw in their lot with the Parliament, why they afterwards joined in the restoration of the King and the Church, which was as plainly written as if you could converse with the people who had written the minutes.

[E. F.]

GENERAL MONTHLY MEETING,

Monday, February 7, 1887.

Henry Pollock, Esq. Treasurer and Vice-President, in the Chair.

Gustav Bischof, Esq. F.C.S.
James Frederick Burton, Esq.
G. Donaldson, Esq.
Harry Montague Elder, Esq. B.A.
Rev. Alfred William Momerie, D.D.
Mrs. Bloomfield Moore,
John Alexander Radcliffe, Esq.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Proceedings, Vol. XXI. Part 2. 8vo. 1886.
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Facts and Fictions in Irish History. By Lord Brabourne. 4to. 1886.
Boston Society of Natural History—Memoirs, Vol. III. Nos. 12, 13. 4to. 1886.
Proceedings, Vol. XXIII. Part 2. 8vo. 1886.
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Cambridge Philosophical Society—Proceedings, Vol. V. Part 6. 8vo. 1886.
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Congregational Library—Catalogue. 8vo. 1886.
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Chemical News for December 1886, January 1887. 4to.
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Engineer for December 1886, January 1887. fol.
Engineering for December 1886, January 1887. fol.
Horological Journal for December 1886, January 1887. 8vo.

Industries for December 1886, January 1887. fol.
Iron for December 1886, January 1887. 4to.
Nature for December 1886, January 1887. 4to.
Revue Scientifique for December 1886, January 1887. 4to.
Telegraphic Journal for December 1886, January 1887. 8vo.
Zoophilist for December 1886, January 1887. 4to.
Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 22, 25. 8vo. 1886.
Franklin Institute—Journal, Nos. 732, 733. 8vo. 1886.
General Medical Council—Third Report of Statistical Committee. 8vo. 1886.
Geographical Society, Royal—Proceedings, New Series, Vol. VIII. No. 12; Vol. IX. No. 1. 8vo. 1886–7.
Johns Hopkins University—Studies in Historical and Political Science, Fourth Series, Nos. 11, 12; Fifth Series, Nos. 1, 2. 8vo. 1886–7.
University Circular, Nos. 53, 54. 4to. 1886.
American Chemical Journal, Vol. VIII. No. 6. 8vo. 1886.
American Journal of Philology, No. 27. 8vo. 1886.
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Lawrence, Edwin, Esq. M.R.I. (*the Author*)—The Progress of a Century. 4to. 1886.
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Mensbrugghe, M. G. Van der (*the Author*)—L'Instabilité de l'équilibre de la Couche Superficielle d'un Liquide. Partie 1e, 2e. 8vo. 1886.
Meteorological Office—Hourly Readings, 1884, Part 1. 4to. 1886.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quarta, Vol. VI. No. 9. 8vo. And Disegni. fol. 1886.
Murray, John, Esq. (*the Editor*)—Murray's Magazine, No. 1. 8vo. 1887.
Numismatic Society—Chronicle and Journal, 1886, Part 3. 8vo.
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Pennsylvania, Second Geological Survey of—Annual Report, 1885. With Atlas. 8vo. 1886.
Pharmaceutical Society of Great Britain—Journal, Dec. 1886, Jan. 1887. 8vo.
Photographic Society—Journal, New Series, Vol. XI. Nos. 2, 3. 8vo. 1886.
Prince, C. Leeson, Esq. (*the Author*)—The Climate of Uckfield, Sussex, and its Neighbourhood, 1843–1870. 2nd edition. 8vo. 1886.
Richardson, B. W. M.D. F.R.S. (*the Author*)—The Asclepiad, Vol. IV. No. 13. 8vo. 1887.
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Victoria Institute—Journal, No. 79. 8vo. 1886.

WEEKLY EVENING MEETING,

Friday, February 11, 1887.

SIR WILLIAM BOWMAN, Bart. LL.D. F.R.S. Manager and Vice-President, in the Chair.

EDWARD B. POULTON, Esq., M.A.

Gilded Chrysalides.

Previous Work.—Mr. T. W. Wood in 1867 published the observation that certain pupæ (*Pieris brassicæ, P. rapæ*, &c.) resemble in colour the surface on which they are found. Although this was disputed by some naturalists, it was confirmed by Mr. A. G. Butler and Prof. Meldola. In 1874 Mrs. M. E. Barber published some very striking observations on the colours of the pupa of *Papilio nireus* (South Africa) confirmation being afterwards afforded by Mr. Trimen, from the case of *Papilio demoleus.* Dr. Fritz Müller, however, shows that *Papilio polydamus* is not sensitive to surrounding colours. The observations were explained by supposing the moist skin of the freshly formed pupa to be "photographically sensitive" to the colour of surrounding surfaces; but Prof. Meldola pointed out that there can be no real analogy with photography. Furthermore, many pupæ are formed at night when the surrounding surfaces are dark. The present investigation was undertaken with the belief that the influence would be found to work upon the larva as it rests upon some coloured surface before pupation.

I. *Experiments upon Vanessa Io.*—This pupa appears in two varieties, being commonly dark grey and much more rarely yellowish-green. Six larvæ placed in a glass cylinder covered with green tissue paper, produced six green pupæ; one of these transferred to a black surface while still moist and fresh, became a green pupa precisely like the others.

II. *Experiments upon Vanessa urticæ.*—The pupæ have no green form, but appear in many shades of dark grey, the lighter ones having golden spots on them, while the extreme forms are almost covered with the golden appearance. These latter are very rarely seen in nature, except when the pupa is diseased. Over 700 pupæ were obtained in the following experiments:—

1. *Effects of Colours.*—*Green* and *orange* surroundings caused no effect on the pupal colours; *black* produced, as a rule, dark pupæ; *white* produced light pupæ, many of them being brilliantly golden. This last result suggested the use of *gilt* surroundings, which were found to be more efficient than white, and produced pupæ with a colour which even more resembled gold.

2. *Mutual Proximity.*—The larvæ being dark, it was found that when many of them became pupæ on a limited (white or gilt) area, the

pupæ were darker than when they had been more isolated. The colours of each were in fact affected by that part of the surroundings made up by the black skins of its neighbours.

3. *Illumination.*—Black surroundings produced rather stronger effects in darkness than in light, but the pupæ were dark in both cases.

4. *Time of Susceptibility.*—The mature larvæ, after ceasing to feed, wander (stage i.) until they find a surface on which to pupate; they then rest upon it (stage ii.), and finally hang, head downwards, suspended by their last pair of claspers (stage iii.), in which position pupation takes place. Stage i. is variable in length, stage ii. may be estimated at 15 hours (but it is also variable), while stage iii. is fairly constant, and lasts about 18 hours; while the whole period is commonly about 36 hours in length. The larvæ are probably affected by surrounding colours for about 20 hours, before the last 12 hours of the whole period, and in this time the pupal colours are determined. These facts were discovered by a very large number of experiments, in which larvæ were placed in surroundings of one colour, and then after a variable time were transferred to another colour producing an opposite effect. It was thus found that stage ii. is more sensitive than stage iii., although there is some susceptibility during the latter stage.

5. *The Part of the Larvæ which is Sensitive to Colour.*

(α) *The Ocelli.*—The most obvious suggestion was that the larval eyes (or ocelli, six on each side of the head) saw the colours, and being influenced, transmitted an impulse to the nervous centres which regulate the formation of the pupal colours. When, however, these organs were covered with black varnish, the pupæ resembled surrounding surfaces to the same extent as when they were produced from normal larvæ.

(β) *The Complex Branching Spines.*—It seemed possible that these structures might contain some organ which was influenced by the colour, but after cutting them off, the larvæ remained normally sensitive.

(γ) *The General Surface of the Skin.*—This was tested by *conflicting colour experiments.* It had been previously shown that the larvæ were sensitive during stage iii., and therefore they were covered in this stage with compartmented tubes, so constructed that the head and anterior part of the body hung in the lower chamber of one colour, while the posterior part of the body was in the upper chamber in another colour. In another method, the larvæ were hung upon a vertical surface, while the head and front part of the body passed through a hole in a shelf, the vertical surface above the shelf, and the upper side of the shelf itself being one colour, while the vertical surface below the shelf and the lower side of the shelf were of the colour tending to produce the most opposite effects. The result of all these experiments was to show that the colour influence does act on some element of the larval skin, and that the larger the area of skin

exposed to any one colour the more does the pupa follow its influence. Particoloured pupæ were not obtained, thus probably pointing towards the action of the nervous system rather than towards the direct action of light on or through the skin itself.

6. *The Nature of the Effects produced.*—The colouring matter of the dark pupæ is contained in a thin superficial layer of the cuticle; below this is a thicker layer divided into exceedingly delicate lamellæ between which fluids are present, and the latter form the thin plates which, by causing interference of light, produce the brilliant metallic appearance. The thinner upper layer being dark, acts as a screen in the dark pupæ. Precisely the same metallic appearances are caused by the films of air between the thin plates of glass which are formed on the surface of bottles long exposed to earth and moisture. Both have the same spectroscopic characters and the same transmitted colours (complementary to those seen by reflection). The brilliancy of the cuticle can be preserved in spirit for any length of time; it disappears on drying, but can be renewed on wetting (this had been previously known), and the colours are seen to change during the process of drying, and when the cuticle is pressed, for the films are thus made thinner. The same lamellated layer exists in non-metallic pupæ, and is used as a reflector for transparent colouring matter contained in its outer lamellæ. Thus the structure which rendered possible the brilliant effects due to interference, probably existed long before these special effects were obtained, and was used for a different purpose.

7. *The Biological Value of the Gilded Appearance.*—It is probable that the gilded pupæ of Vanessidæ resemble glittering minerals such as mica (which is very common in many places); their shape is very angular, and like that of minerals: conversely the grey pupæ resemble grey and weathered rock-surfaces, and the two conditions of rock would themselves act as a stimulus for the production of pupæ of corresponding colour. The power was probably gained in some dry hot country, where mineral surfaces do not weather quickly. Once formed it may be used for other purposes, and in certain species is probably a warning to the enemies that the insect is inedible. It is interesting to note how the Vanessidæ, primarily coloured so as to resemble mineral surroundings, are modified for pupation on plants. Thus *Vanessa Io* has a green form which is produced among leaves; *V. atalanta* has no green form, and spins together the leaves for concealment, but both these species commonly pupate freely exposed on mineral surfaces; *V. urticæ* has neither the green form nor the habit, and it has a strong disinclination to pupate on its food-plant, as many observations concurred in proving.

III. *Experiments upon Vanessa atalanta.*—This species was also made brilliantly golden or dark-coloured by the use of appropriate surroundings in the larval condition.

IV. *Experiments upon Papilio machaon.*—This species, like *P. polydamus* (Fritz Müller) has no power of being influenced by surrounding colours. A brown pupa was obtained on the food-plant, and many

green ones upon brown twigs, &c. It is probable that less healthy and smaller larvæ often produce the brown form, just as diseased Vanessa larvæ produce gilded pupæ.

V. *Experiments upon Pieris brassicæ and P. rapæ.*

1. *Effects of Colours*—*Black* produced dark pupæ, and the greater the illumination the darker the pupæ (*P. rapæ*), this result being the reverse of that obtained with *V. urticæ*; *white* produced light pupæ, and the greater the illumination the lighter the pupæ (*P. rapæ*); *dark red* (*P. brassicæ*) produced dark pupæ; *deep orange*, in both species, produced very light pupæ of a green colour; *pale yellow* and *yellowish-green* produced rather darker pupæ than the orange; *bluish-green* produced much darker pupæ, while *dark blue* produced still darker pupæ (*P. rapæ* only). Hence there is a remarkable and sudden fall, followed by a slow and gradual rise in the amount of pigment formed as the light from various parts of the spectrum from red to blue predominates in the reflected rays which fall on the larval surface. But their effects on the formation of superficially placed dark pigment are accompanied by changes affecting the formation of greens and yellows, &c., in the deeper subcuticular tissues. Hence the results of any given stimulus are exceedingly complicated.

2. *Other Experiments.*—It was shown by the method described above that the ocelli are not sensitive in this species, and by similar transference experiments it was proved that the influence acts on the larva and not on the pupa itself.

VI. *Experiments upon Ephyra pendularia.*—In this genus of moths the exposed pupæ are often green and brown in different individuals, but these colours follow the corresponding tints of the larvæ, and therefore cannot be influenced unless the latter themselves were changed, and such susceptibility in the larval state has not been proved for this genus. This is the only known instance of a constant relation between the larval and pupal colours.

VII. *Experiments upon the Cocoon of Saturnia carpini.*—It was found that the larvæ spin dark cocoons in black surroundings, but white ones in lighter surroundings.

[E. B. P.]

WEEKLY EVENING MEETING,

Friday, February 18, 1887.

Sir Frederick Abel, C.B. D.C.L. F.R.S. Manager and Vice-President, in the Chair.

William Crookes, Esq. F.R.S. V.P.C.S. *M.R.I.*

Genesis of the Elements.

In the very words selected to denote the subject I have the honour of bringing before you, I have raised a question which may be regarded as heretical. At the time when our modern conception of chemistry first dawned upon the scientific mind, the average chemist as a matter of course accepted the elements as ultimate facts. He regarded his elements as absolutely simple, incapable of transmutation or decomposition, each a kind of barrier behind which we could not penetrate. If closely pressed he said that they were self-existent from all eternity, or that they had been individually created just as we now find them at the present day. Or he might argue that the origin of the elements did not in the least concern us, and was, indeed, a question lying outside the boundaries of science.

But in these our times of restless inquiry we cannot help asking what are these elements, whence do they come, what is their signification? We cannot but feel that unless some approach to an answer to these questions can be found, our chemistry, after all, is something profoundly unsatisfactory. These elements perplex us in our researches, baffle us in our speculations, and haunt us in our very dreams. They stretch like an unknown sea before us—mocking, mystifying, and murmuring strange revelations and possibilities.

If I venture to say that our commonly received elements are *not* simple and primordial, that they have *not* arisen by chance or have *not* been created in a desultory and mechanical manner but have been evolved from simpler matters—or perhaps indeed from one sole kind of matter—I do but give formal utterance to an idea which has been, so to speak, for some time "in the air" of science. Chemists, physicists, philosophers of the highest merit declare explicitly their belief that the seventy (or thereabouts) elements of our text-books are not the pillars of Hercules which we must never hope to pass.

Did time allow I might quote utterances of Dalton, of Professor Faraday, of Dr. Gladstone, of the late Sir Benjamin Brodie, of Professor Graham, of Dr. Mills, of Professor Stokes, of Mr. Norman Lockyer, all pointing in the same direction and all showing that in the course of their researches these servants of Science have been led to think that these same elements are not the final outcome—the be-all and the end-all of chemistry.

The law of Prout, and still more the better established and far-reaching periodic law of Newlands (since developed by Professors Mendeleeff, Meyer, and Carnelley), seem to presuppose the existence of a genetic relation among the elements.

Philosophers in the present as in the past,—men who certainly have not worked in the laboratory,—have reached the same view from another side. Thus Mr. Herbert Spencer records his conviction that "the chemical atoms are produced from the true or physical atoms by processes of evolution under conditions which chemistry has not yet been able to produce."

And the poet has forestalled the philosopher. Milton ('Paradise Lost,' Book V.) makes his Archangel Raphael say to Adam, instinct with the evolutionary idea, that the Almighty had created

> "one first matter all,
> Indued with various forms, various degrees
> Of substance."

If we can show how the so-called chemical elements might have been generated we shall be able to fill up a formidable gap in our knowledge of the universe. We have a preponderance of cumulative evidence to prove that both heavenly bodies and living organisms have been formed by evolution. We are seeking now to extend this law to the so-called elements, to the first principles of which stars and organisms alike consist.

If we survey the distribution of the chemical elements we find two very distinct cases. On the one hand we see bodies grouped in definite proportions with other bodies from which they differ exceedingly and to which they are held by affinity, more or less strong. To obtain either of two such bodies in a separate state, that affinity, as every student of chemistry knows, must be overcome. Instances of such association are too common and abundant to need mention. In such cases each of the bodies grouped together has fairly marked properties. One of them, moreover, for the most part has an atomic weight very different from that of the other.

In the second case we find bodies associated with other bodies more or less closely allied to themselves. They are not held together by any decided affinity; they are not combined in definite proportions, and their atomic weights are often almost identical. If we wish to obtain one or more of these bodies in a separate state, the difficulty encountered lies not in the strength of the affinities to be overcome but in the circumstance that whatever reagent we employ acts upon one of the substances in nearly the same manner as it does upon the other. Hence, to obtain one body of this kind entirely separate is an exceedingly tedious and difficult task. Nay, we are sometimes at a loss to decide whether we have before us a really simple body or a mixture of bodies whose properties are almost identical.

The most striking instance of such association is found in the

metals of the so-called *rare earths.* These bodies form but a very trifling portion of the earth's crust. They are chiefly met grouped together in a few minerals, such as samarskite and gadolinite, which, so far, have been found in but few localities, and even in those are far from common. These earths form a group to themselves; chemically, they are so much alike that it taxes the utmost skill of the chemist to effect even a partial separation, and their history is so obscure that we do not yet know the number of them.

It will not be necessary here to explain in detail the process of chemical fractionation adopted for the separation of the rarer earths, since it could interest only the chemical specialist; moreover, it has been fully described in a paper I read before the British Association at Birmingham.

Stated in the briefest way the operation consists in fixing upon some chemical reaction in which there is the most likelihood of a difference in the behaviour of the elements under treatment, even though the difference be slight, and effecting such treatment incompletely, so that only a certain fraction of the total bases present is separated: the object being to get part of the material in an insoluble and the remainder in a soluble state.

Let us suppose that we have in solution two earths almost identical in their properties, but differing slightly, almost imperceptibly, in basicity. We add to the solution of the earths, which must be very dilute, weak ammonia to such an amount only that it precipitates one-half of the bases present. The dilution must be so great that a considerable time must elapse before the liquid shows a turbidity, and several hours will have to pass over before the action of the ammonia is complete. The liquid is then filtered, by which process we have the earths divided into two parts, no longer identical in their composition. We can easily see that there is now a slight difference in the basic value of the two portions of earths; the portion in solution being, though by a scarcely perceptible amount, more basic than that which the ammonia has precipitated. This minute difference is made to accumulate systematically until it becomes perceptible either by chemical or physical tests.

The accompanying diagram (Fig. 1), illustrates the scheme of fractionation. Starting from zero at the apex the precipitates all pass to the left and the filtrates to the right. Each circle represents a flask containing the solution under treatment, and the two arrows from each circle show the path pursued respectively by the precipitate and filtrate.

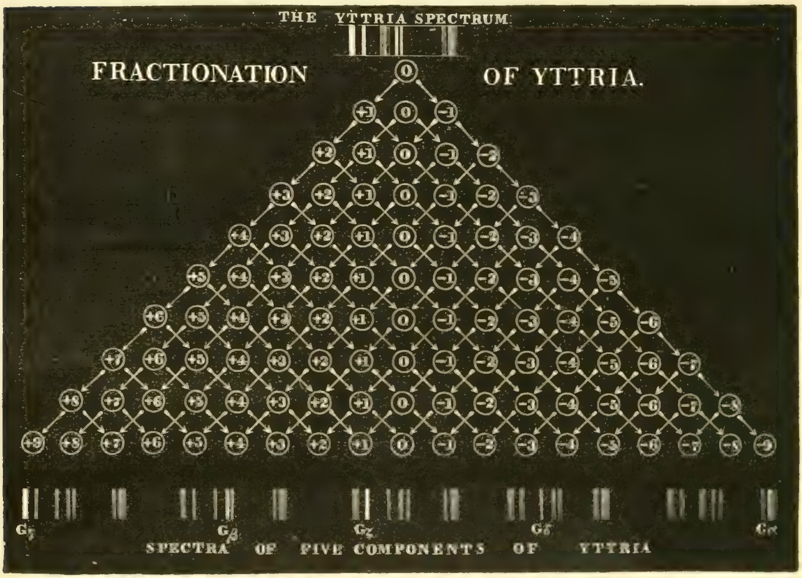
Such is the general outline of the process. But, as has been already intimated, the methods of separation suitable for different groups of earths vary. Where the constituents of yttrium and samarium are concerned, nothing seems available but straightforward fractionation continued month after month and year after year.

The further question whether an earth we have separated is really simple or is still a mixture has again to be decided by yet another

process, to be effected only in a very high vacuum. To understand this process it is necessary to make an apparent total digression.

It seems, perhaps, strange to speak of exhausting the air in hollow bulbs and tubes until there is left in them only the one-millionth

FIG. 1.



part of an atmosphere. It is only in modern times that atmospheric air has come to be regarded as matter. To this day a bottle or a jar is said to be "empty" if it contains no liquid or solid body, the air with which it is filled being completely ignored. According to the same common idea, how empty then must a vessel be when the air it contains is reduced to the one-millionth part of its original quantity! That something still remains is, however, proved by the fact that I have succeeded in reducing the pressure down to one fifty-millionth of an atmosphere. What this number represents will be better understood if I say that, given a barometric column one hundred miles in height, the remaining pressure would be equal only to about the tenth of an inch. Even this high degree of exhaustion by no means represents an absolute vacuum. I have in this glass tube perhaps the nearest approach to perfect emptiness yet artificially obtained. Its internal capacity is 5 cubic centimetres, and it is exhausted to the one fifty-millionth part of an atmosphere. It still contains 100,000000,000000 molecules. The internal space, therefore, is far, very far, from being absolutely void of matter.

The vacuum most suitable for experiments on these earths is one of about the millionth of an atmosphere. In a vacuum of this degree we find under the action of the induction-spark certain substances phosphorescing or behaving very differently from what they would if similarly treated at a lower vacuum or at the ordinary pressure of the atmosphere. When thus treated, the examination of the spectra of the phosphorescing earths furnishes what I have ventured to call the radiant matter test.

After a time, on examining the series of yttrium earths in the lowest line of flasks, their phosphorescent spectra are found to have become modified in the relative intensities of some of their lines. Ultimately different portions of the fractionated yttria give the five-spectra approximately shown at the foot of the diagram; whilst samaria also appears capable of being split up into two or perhaps three constituents.

These bodies, it must be clearly understood, are not impurities which may be removed, yttrium or samarium remaining in a pure state after their elimination. On the contrary, the molecule we formerly knew as yttrium has undergone a veritable splitting up into its constituents.

These constituents I have not as yet formally baptised. For more convenient reference and discussion I have provisionally ticketed them, as shown in the following Table.

TABLE I.

Position of Lines in the Spectrum.	Scale of Spectroscope.	Mean Wave-length of Line or Band.	$\frac{1}{\lambda^2}$	Provisional Name.	Probability.
Bright lines in—					
Violet	8·515	456	4809	$S\gamma$	New, or ytterbium.
Deep blue	8·931	482	4304	$G\alpha$	New.
Greenish blue (mean of a close pair) ..	9 650	545	3367	$G\beta$	New, or the $Z\beta$ of M. de Boisbaudran.
Green	9·812	564	3144	$G\gamma$	New.
Citron	9·890	574	3035	$G\delta$	New, or the $Z\alpha$ of M. de Boisbaudran.
Yellow	10·050	597	2806	$G\epsilon$	New.
Orange	10·129	609	2693	$S\delta$	New.
Red	10·185	619	2611	$G\zeta$	New.
Deep red	10·338	647	2389	$G\eta$	New.

I will rapidly sketch the most salient features of the rare earths when submitted to the radiant matter test. Some remain unaffected and thus are referred at once to a distinct group. Others have the curious property of preventing the induction-spark passing, and so simulating a non-conducting vacuum, when there is really plenty of residual gas present. The rare earth thoria possesses in the highest

degree this obstructive property. Before me I have an exhausted tube having two sets of poles sealed in it, one set at each end. The size and distance apart of these poles are exactly the same in each case. At one end of the tube I have put some thorium sulphate, at the other end I have put yttrium sulphate. The exhaustion is now proceeding by aid of the Sprengel pump. I attach the wires of the induction coil to the poles at the thorium end, and, as you see, no current will pass; rather than pass through the tube, the spark prefers to strike across the spark-gauge in air—a striking distance of 37 millimetres,—showing an electromotive force of 34,040 volts. Now, without doing anything to affect the degree of exhaustion, I transfer the wires of the induction coil from the thorium to the yttrium end, and the spark passes at once. To balance the spark in air I must push the wires of the gauge together, till they are only 7 millimetres apart, equivalent to an electromotive force of 6440 volts: the fact of whether thoria or yttria is under the poles making a difference of 27,600 volts in the conductivity of the tube. The explanation of this eccentric action of thoria is not yet quite clear. From the great difference in the phosphorescence of the two earths, it is evident that the passage of electricity through these tubes is not so much dependent on the degree of exhaustion as upon the phosphorogenic property of the body opposite the poles.

Other earths become very phosphorescent, and their power of retaining residual phosphorescence differs greatly among themselves. This property we shall presently see is one of some importance. To examine this persistence of luminosity I have devised an instrument similar to Becquerel's phosphoroscope, but acting electrically instead of by means of direct light. It consists of an opaque disc, 30 inches in diameter, pierced with six openings near the edge. By means of a multiplying wheel and pulley the disc can be set in rapid rotation. At each revolution a stationary object behind one of the apertures is alternately exposed and hidden six times. A commutator forms part of the axis of the disc, and by connecting it with the wires from a battery, rotation of the disc produces alternate makes and breaks in the current. This primary current is then connected with the induction coil, from which the secondary current passes through the vacuum tube containing the earth under examination. When a phosphorescent body such as yttria is examined, if the wheel is turned slowly no light is seen when looked at from the front, as the current does not begin till the obscuration of the tube by an intercepting segment, and ends before the earth comes into view. When, however, the wheel is quickly turned, the residual phosphorescence lasts long enough to bridge over the brief interval between the cessation of the spark and the entry of the phosphorescent body into the field of view, and it is seen to glow with a faint light which becomes brighter as the speed of the wheel increases.

I will first put the phosphorescent earth glucina in the phosphoroscope. This phosphoresces of a bright blue colour, but the

residual glow is so short that, with the highest speed of which the instrument is capable, you see no light whatever. In contrast I now put in a compound of the earth strontia. This also glows with a rich blue colour, showing in the spectroscope a continuous spectrum with a great concentration of light in the blue and violet. In the phosphoroscope the colour of the glow is bright green, showing in the spectroscope a continuous spectrum, with the red and blue ends cut off.

Alumina in the radiant matter tube glows with a rich crimson light. I will put some rubies—a crystalline form of alumina—in the phosphoroscope. Here the persistence of luminosity is so great that the red light is visible with the slowest speed, and with a high velocity the residual glow is nearly as strong as when the rubies are out of the instrument. Shakespeare, who is supposed to have mastered all knowledge, had he seen these rubies could hardly have described them more precisely than in the lines from 'Julius Cæsar' :—

> " . . . with unnumbered sparks
> They are all fire, and every one doth shine."

Another distinctive phenomenon is that the earths of one group, yttrium and samarium, when submitted to the induction discharge *in vacuo*, yield discontinuous spectra.

These spectra are extremely complicated and change in their details in a puzzling manner. For many years I have persistently groped on in almost hopeless endeavour to get a clue to the meaning which I felt convinced was locked up in these systems of bands and lines. It was impossible to divest myself of the conviction that I was looking at a series of autograph inscriptions from the molecular world, evidently of intense interest, but written in a strange and baffling tongue. For a long time all attempts to decipher these mysterious signs were fruitless.

The meaning of the strongly-marked symbolic lines had first to be ascertained. After continued efforts I had to be content with roughly translating one group of coloured symbols as " yttrium " and another group as " samarium," disregarding the fainter lines, shadows, and wings frequently common to both. Constant practice in the decipherment has now given me fuller insight into what I may call the grammar of these hieroglyphic inscriptions. Every line and shadow of a line, each faint wing attached to a strong band, and every variation in intensity of the shadows and wings among themselves, has now a definite meaning which can be translated into the common symbolism of chemistry.

This leads us to what I may call the history of yttrium. Twelve months ago the name yttrium conveyed to all chemists a perfectly definite meaning. It was supposed to be an elementary or simple body, having a fixed atomic weight, 88·9, and its principal properties had been duly determined. Its phosphorescent spectrum gave a definite system of coloured bands, such as you see in the drawing

before you (Fig. 2). Broadly speaking, there is a deep red band, a very luminous citron-coloured band, a pair of greenish-blue bands, and a blue band. These bands, it is true, varied slightly in relative intensities and in sharpness with almost every sample of yttria

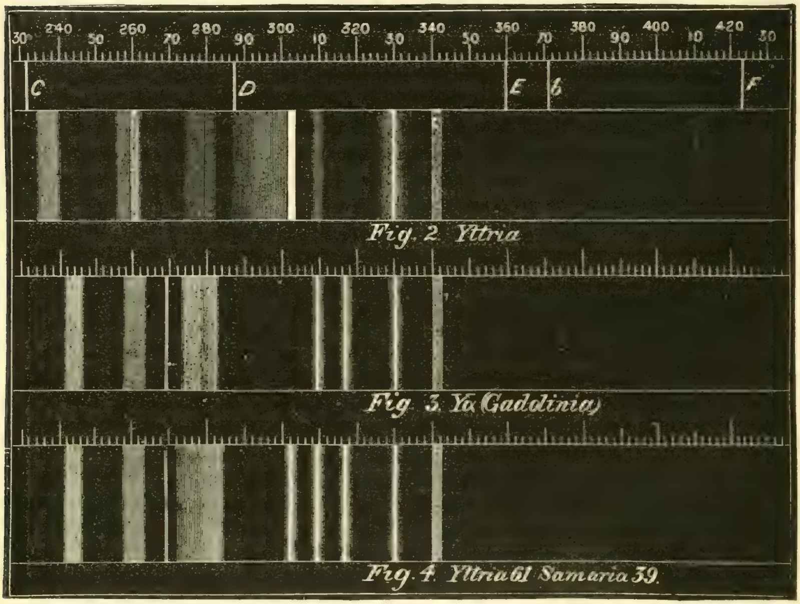


Fig. 2. Yttria

Fig. 3. Yα (Gadolinia)

Fig. 4. Yttria 61 Samaria 39.

I examined; yet the general character of the spectrum remained unchanged, and I habitually looked upon this spectrum as characteristic of yttrium; all the bands being visible when the earth was present in quantity, whilst only the strongest of all—the citron band—was visible when traces, such as millionths, were present. But that the whole system of bands spelled yttrium, and nothing but yttrium, I was firmly convinced.

The differences in the spectra of yttrium prepared from different sources are most distinctly seen on comparing the spectrum of yttrium from samarskite with that from gadolinite, hielmite, monazite, xenotime, fluocerite, euxenite, cerite, arrhenite, &c. Still, in spite of these slight differences, the several yttriums are practically all the same thing, and, as I have said, every living chemist a year ago would have regarded them as identical. But they have since yielded to persistent chemical fractionation, and I now call them old yttrium.

One property, above all others, is relied on by chemists as an indisputable proof of the identity of any particular chemical element.

When the vapour of an element is rendered incandescent by the electric spark, the characteristic system of lines in its spectrum is regarded as unalterable, and is looked upon as a certain proof that this special element is under examination. However much chemical or other tests fail to show the presence of a given element, the indications of its lines in the spectroscope are regarded as infallible. Spectrum analysis is the court of final appeal, whose decision no chemist has yet had the hardihood to dispute.

By way of illustration I will project on the screen the very characteristic system of lines given by yttrium when ignited by the electric spark,—a system, be it remembered, having no connection whatever with the peculiar phosphorescent spectrum yielded by yttrium. The coloured diagram gives as accurate a representation of the spark spectrum of yttrium as can be drawn by hand. Omitting minor lines you will notice two very strong groups of lines in the red and orange. These lines have been always regarded as the characteristic test for yttrium; the presence of these groups proves the presence of yttrium, and their absence proves its absence.

I now project the electric spark spectrum of $G\delta$ as pure as I have been able to prepare it. $G\delta$ is one of the bodies which by long and tedious fractionation I have separated from yttrium; it occurs at one extreme end of the fractioning, and differs not only from the parent yttrium in its phosphorescent spectrum, but by virtue of the process adopted for its isolation, it must likewise differ in chemical properties. But what tale does the spectrum tell? It tells us there is absolutely no difference between this spectrum and that given by old yttrium.

I now pass to the other end of the fractionation of yttrium, where a body, $G\eta$, concentrates giving a totally different phosphorescent spectrum to that given by $G\delta$. And it also differs chemically from old yttrium, and in a more marked manner from its brother, $G\delta$, at the other extremity of the fractionation. Look at its spark spectrum! It is perfectly identical both with old yttrium and with $G\delta$, and when I examine these three spectra in my laboratory with all the appliances for exact measurement, the whole system of lines is still identical.

What inference can be drawn from these results? Is discredit to be thrown on spectrum analysis? Is the superstructure which has been so laboriously raised upon its indications to fall to the ground? By no means. Spectrum analysis and its grand generalisations are on as firm a foundation as ever. I see two possible explanations of the facts I have brought before you. According to one hypothesis research has somewhat enlarged the field lying between the indications given by ordinary coarse chemistry and the searching scrutiny of the prism. Our notions of a chemical element have expanded. Hitherto the molecule has been regarded as an aggregate of two or more atoms, and no account has been taken of the architectural design on which these atoms have been joined. We may consider that the structure of a chemical element is more complicated than has hitherto been

supposed. Between the molecules we are accustomed to deal with in chemical reactions and ultimate atoms as first created, come smaller molecules or aggregates of physical atoms; these sub-molecules differ one from the other, according to the position they occupied in the yttrium edifice.

Perhaps this hypothesis can be simplified if we imagine yttrium to be represented by a five-shilling piece. By chemical fractionation I have divided it into five separate shillings, and find that these shillings are not counterparts, but like the carbon atoms in the benzol ring, have the impress of their position, 1, 2, 3, 4, 5, stamped on them. These are the analogues of my $G\alpha$, $G\beta$, &c. If I now bring in a much more powerful and searching agent—if I throw my shillings into the melting-pot or dissolve them chemically—the mint stamp disappears and they all turn out to be silver. I submit my yttrium, or my $G\alpha$, $G\beta$, &c., to the intense heat of the electric spark, the little differences of molecular arrangement vanish, and the atoms of which the molecules of yttrium, $G\alpha$ and $G\beta$, are alike composed, reveal their presence in identical spectra.

An alternative theory commends itself to chemists, to the effect that the nine bodies shown in the above table (Table I.), are new chemical elements differing from yttrium and samarium in basic powers and several other chemical and physical properties, but not sufficiently to enable us to effect any but a slight separation. One of these bodies, $G\delta$, gives the phosphorescent citron line, and also the brilliant electric spectrum I have just exhibited. The other eight do not give electric spectra which can be recognised in the presence of a small quantity of $G\delta$, whilst the electric spectrum of $G\delta$ is so sensitive that it shines out in undiminished brilliancy even when the quantity present is extremely minute. In the process of fractionation, $G\alpha$, $G\beta$, $G\delta$, &c., are spread out and more or less separated from one another, yet the separation is imperfect at the best, and at any part there is enough $G\delta$ to reveal its presence by the sensitive electric spark test. The arguments in favour of each theory are strong and pretty evenly balanced. The compound molecule explanation is a good working hypothesis, which I think may account for the facts, while it does not postulate the rather heroic alternative of calling into existence eight or nine new elements to explain the phenomena. However, I submit it only as an hypothesis. If further research shows the new element theory is more reasonable, I shall be the first person to accept it.*

* Neither of these theories agrees with that of my distinguished friend M. Lecocq de Boisbaudran, who also has worked on these earths for some time. He considers that what I have called old yttrium is a true element, characterised by the spark spectrum already exhibited, but not giving a phosphorescent spectrum *in vacuo*. The bodies giving the phosphorescent spectra he considers to be impurities in yttrium. These he says are two in number, and he has provisionally named them $Z\alpha$ and $Z\beta$. By a method of his own, differing from mine, M. de Boisbaudran obtains fluorescent spectra of these bodies; but their

I now will introduce to you a substance which has been to me what the celebrated Rosetta stone was to the interpreters of Egyptian inscriptions. I received it from M. de Marignac, and it was nothing more than a small specimen of a new earth which he had obtained and had named provisionally Yα.

In the radiant-matter tube this earth gives a bright spectrum as in the diagram before you (Fig. 3).

If we compare this spectrum with that ascribed to "old yttrium" (Fig. 2) we see that, omitting minor details, Yα is yttrium with the characteristic citron band left out and the green and orange bands of samarium added. Now look at the following diagram (Fig. 4), which represents the spectrum of a mixture of 61 parts of yttrium and 39 parts of samarium. It is almost to its minutest details identical with the spectrum of Yα, but the citron band is as prominent as any other band. Hence Yα is shown to consist of samarium, with the greenish blue of yttrium and some of the other yttrium bands added to it. It proves, further, that the citron band which I had hitherto regarded as one of the essential bands of the yttrium spectrum can be entirely removed, whilst another characteristic yttrium group, the double green band, can remain with heightened brilliancy.

If now it were possible to remove the citron band-forming body from this mixture, I should leave Yα behind; I should, in fact, have recomposed Yα from its elements. I have no doubt whatever that this will ultimately be accomplished, but the preliminary work of fractionation is tedious to the last degree, and for its completion would occupy a space of time in comparison with which the life of man is all too brief.

Whilst I have not yet chemically removed the citron-forming constituent, I can physically suppress the citron band and show an artificial spectrum, imitating in the closest degree the natural spectrum of Yα.

By means of the electrical phosphoroscope I am enabled to catch the spectrum of an earth immediately after it has suffered molecular bombardment in the vacuum. In this way I get the spectrum of the residual phosphorescence, and I have found that not all the constituents of these earths emit residual phosphorescence for the same duration of time.

When a little strontium is added to the yttrium-samarium mixture, the effect in the phosphoroscope is to suppress the residual phosphor-

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fluorescent bands are extremely hazy and faint, rendering identification difficult. Some of them fall near lines in the spectra of my Gβ and Gδ. At first sight it might appear that his and my spectra were due to the same bodies, but, according to M. de Boisbaudran, the chemical properties of the earths producing them are widely distinct. Those giving phosphorescent lines by my method occur at the yttrium extremity of the fractionation, where his fluorescent bands are scarcely shown at all; whilst his fluorescent phenomena are at their maximum quite at the terbium end of the fractionation, where no yttrium can be detected even by the direct spark, and where my phosphorescent lines are almost absent.

escence of Gδ—the citron band—and to enhance the phosphorescence of Gβ, the double green band, and the imitation of the Yα spectrum is complete.

I must here call attention to the experiments of Prof. A. E. Nordenskiöld, in the *Comptes Rendus* of the French Academy of Sciences for November 2nd, 1886. This eminent savant is working in the same direction as myself, with results which decidedly corroborate my experiments. He has taken the crude mixture of yttria, erbia, ytterbia, &c., just as it is precipitated from the minerals containing these rare earths. This mixture, for brevity's sake, he calls gadolinia, and he finds that this gadolinia, though palpably a compound body, has always a constant atomic weight, whatever the mineral from which it has been extracted. Or, to use Prof. Nordenskiöld's own words, "*Oxide of gadolinium, though it is not the oxide of a simple body, but a mixture of three isomorphous oxides* (even when it is derived from totally different minerals found in localities far apart from each other) *possesses a constant atomic weight.*" Therefore, as he significantly observes, "*We are in presence of a fact altogether new in chemistry.* For the first time we are confronted with the fact that three isomorphous substances, of a kind that chemists are still compelled to regard as elements, occur in nature not only always together, but in the same proportions. It seems that chemists here find themselves face to face with a problem analogous to that presented to astronomers in the origin of the minor planets."

These facts throw a new light upon certain important chemical questions. For the old yttrium passed muster as an element. It had a definite atomic weight, it entered into combination with other elements, and could be again separated from them as a whole. But now we find that excessive and systematic fractionation has acted the part of a chemical "sorting Demon," distributing the atoms of yttrium into groups, with certainly different phosphorescent spectra, and presumably different atomic weights, though, from the usual chemical point of view, all these groups behave alike. Here, then, is a so-called element whose spectrum does not emanate equally from all its atoms; but some atoms furnish some, other atoms others, of the lines and bands of the compound spectrum of the element. Hence the atoms of this element differ probably in weight, and certainly in the internal motions they undergo.

This is unlikely to be an isolated case. We may assume that the principle is of general application to all the elements. In some, possibly in all elements, the whole spectrum does not emanate from all their atoms, but different spectral rays may come from different atoms, and in the spectrum as we see it all these partial spectra are present together. This may be interpreted to mean that there are definite differences in the internal motions of the several groups of which the atoms of a chemical element consist. For example, we must now be prepared for some such events as that the seven series of bands in the absorption-spectrum of iodine may prove not all to

emanate from every molecule, but that some of these molecules emit some of these series, others others, and in the jumble of all these molecules, to which is given the name "iodine vapour," the whole seven series are contributors.

Another important inference to be drawn from the facts is that yttrium atoms, though differing, do not differ continuously, but *per saltum.* We have evidence of this in the fact that the spectroscopic bands characteristic of each group are distinct from those of other groups, and do not pass gradually into them. We must accordingly expect, in the present state of science, that this is probably the case with the other elements. And the atoms of a chemical element being known to differ in one respect may differ in other respects, and presumably do somewhat differ in mass.

Returning, after this digression, to the idea of heavy and light atoms, we see how well this hypothesis accords with the new facts here brought to light. From every chemical point of view the stable molecular group, yttrium, behaves as an element. To split up yttrium requires not only enormous time and material, but the existence of a test by means of which the constituents of yttrium are capable of recognition. Had we tests as delicate for the constituent molecular groups of calcium, this element also might be resolved into simpler groupings. It is one thing, however, to find out means of separating bodies which we know to be distinct and to have colour or spectrum reactions to guide us at every step; it is quite another thing to separate colourless bodies which are almost identical both in chemical reaction and atomic weight, especially if we have no suspicion that the body we examine is a mixture.

Again, it seems as if bodies we have been accustomed to regard as absolutely simple and elementary may be split up in different directions according to the means we bring to bear upon them. Until very lately our text-books made mention of an element under the name of didymium. With some trouble it had been separated from its accompanying bodies lanthanum and cerium. Its properties had been examined, and no one doubted its distinct and elementary character. It was viewed according to one of the common definitions of an element, as "a something to which we can add, but from which we can take nothing." When, behold! Dr. Auer von Welsbach, examining this supposed simple body in a novel manner, succeeded in decomposing it into two simpler bodies, which he called neodymium and praseodymium; and later researches, in which I have had a share, show that even neodymium and praseodymium are not the simplest bodies into which didymium can be dissected.

But it may be asked, What is the bearing of all this upon the great question of the genesis of the elements? Have we chemists merely discovered some new "elements," or found out that a body hitherto held to be simple is in reality complex? We have, I submit, done something decidedly different. If a metal which is found to have a fixed atomic weight is discovered to be a compound or a

mixture, our best test for recognising an element, so-called, has melted away! Hitherto it has been considered that if the atomic weight of a metal, determined by different observers, setting out from different compounds, was always found to be constant (within, of course, the limits of experimental error), then such metal must rightly take rank among the simple or elementary bodies. We learn from Nordenskiöld's gadolinium that this is no longer the case. Again, we have here wheels within wheels. Gadolinium is not an element, but a compound, or rather, perhaps, a mixture of yttrium, erbium, and ytterbium. We have shown that yttrium is a complex of five or more new constituents. And who shall venture to gainsay that each of these constituents, if attacked in some different manner, and if the results were submitted to a test more delicate and searching than the radiant-matter test, might not be still further divisible? Where, then, is the actual ultimate element? As we advance it recedes like the tantalising mirage lakes and groves seen by the tired and thirsty traveller in the desert. Are we in our quest for truth to be thus deluded and baulked? The very idea of an element, as something absolutely primary and ultimate, seems to be growing less and less distinct.

But we have by no means done with the rare earths and their lessons. How is it that these bodies are found, as we actually find them, associated in certain rare minerals such as samarskite and gadolinite, but occurring only in a few localities? This fact is hard to account for on the ordinary theories of the origination of the elements.

I venture provisionally to conclude that our so-called elements or simple bodies are, in reality, compound molecules. To form a conception of their genesis I must beg you to carry your thoughts back to the time when the visible universe was "without form and void," and to watch the development of matter in the states known to us from an antecedent something. What existed anterior to our elements, before matter as we now have it, I propose to name *protyle*.*

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* We require a word, analogous to protoplasm, to express the idea of the original primal matter existing before the evolution of the chemical elements. The word I have ventured to use for this purpose is compounded of πρό (*earlier than*) and ὕλη (*the stuff of which things are made*). The word is scarcely a new coinage, for in the 'Wisdom of Solomon' (xi., v. 17) we read:—"Thy almighty hand, that created the world—ἐξ ἀμόρφου ὕλης—out of formless stuff," the word here rendered "stuff" being in the original ὕλης, from which I have ventured to coin the word "protyle." Six hundred years ago Roger Bacon wrote in his *De Arte Chymiæ*, "The elements are made out of ὕλη, and every element is converted into the nature of another element." Professor Huxley reminds me that ὕλη, in the general sense of material substance, was first used by Aristotle, in whose works it is of very frequent occurrence. In fact the fundamental distinction in his Physical Philosophy is between ὕλη, or *matter*, and εἶδος, or *form*, which last pretty nearly answers to what we should call the sum total of the qualities, powers, and tendencies of a thing—or of forces as the cause of these. In the metaphysics and elsewhere Aristotle distinguishes (1) Πρώτη ὕλη, "Materia

But how can we suppose the protyle, or fire-mist, converted into the atomic condition? In amorphous matter we recognise a tendency to aggregation not to be identified with gravitation, since it is manifested among finely-divided matter, whether suspended in a medium of a specific gravity superior, equal, or inferior to its own. This agglutinative action is familiar to observers of natural phenomena. Clouds contracting to that appearance known as a mackerel sky; particles of carbon floating in the air, collecting, and ultimately falling as "blacks"; chemical precipitates, at first finely amorphous, but gradually becoming flocky, granular, and crystalline; vortex rings, suddenly quickening out of amorphous smoke;—all these, and many more, exemplify that universal formative principle in nature which I suggest first made itself manifest in the condensation of protyle into atomic matter.

A few weeks ago, in this theatre, Sir William Thomson asked you to travel back with him an imaginary excursion of about twenty million years. He pictured to you the moment immediately before the birth of our sun, when the Lucretian atoms rushed from all parts of space with velocities due to mutual gravitation, and, clashing together, formed in a few hours an incandescent fluid mass, the nucleus of a solar system with thirty million years of life in it. I will ask you to accompany me to a period even more remote,—to the very beginnings of time, before even the chemical atoms had consolidated from the original *protyle*. Let us imagine that at this primal stage all was in an ultra-gaseous state—a state differing from anything we can now conceive in the visible universe.

Now unless the expression "fire-mist" and the supposition that pristine matter was once in an intensely heated condition * are quite misleading and baseless, we have to deal with a process analogous to cooling. This operation, probably internal, reduces the temperature of the cosmic *protyle* to a point at which the first step in granulation takes place; matter as we know it comes into existence, and atoms are formed. As soon as an atom is formed out of *protyle* it is a store of energy, kinetic (from its internal motions), and potential (from its tendency to coalesce with other atoms by gravitation or chemically). To obtain this energy the neighbouring *protyle* must be laid under contribution, i. e. must be refrigerated by it, thereby accelerating the subsequent formation of other atoms. With the birth of gravitating

Prima," or matter undifferentiated into elements, without form, in fact, and consequently *αγνωστος*, unknown and unknowable, and (2) *ἐσχατη ὕλη*. secondary or formed matter, such as earth, or metal, or water, or any other raw material with which we are familiar.

* I am constrained to use words expressive of high temperature; but I confess I am unable clearly to associate with *protyle* the idea of hot or cold. *Temperature*, *radiation*, and *free cooling* seem to require the periodic motions that take place in the chemical atoms; and the introduction of centres of periodic motion into protyle would involve its being so far changed into chemical atoms. Probably the first operation was more analogous to the formation of vortex rings than to a reduction of temperature.

matter, rushing suddenly together from every point of space, we thus get Sir William Thomson's incandescent mass which is presently to cool down into a solar system. We cannot tell if electricity existed prior to the origin of the atomic condition of matter, but with the formation of atomic matter the other forms of energy which require matter in order to manifest themselves, begin to act, amongst others that form of energy which has for one of its factors that which we now speak of as atomic weight.

We have now to seek how protyle was converted not into one only kind of matter but into many. If we recognise that it contained within itself the potentiality of all atomic weights, how did these potentialities become actual? We may here call to mind the suggestion of Dr. E. J. Mills, that our elements are the result of successive polymerisations during the cooling process. We shall also derive much assistance from a method of illustrating the periodic law proposed by my friend Professor Emerson Reynolds, of the University of Dublin.

I must call your attention to a diagram (Fig. 5) in which I have slightly modified the original design of Professor Reynolds. I have represented the pendulum swing as gradually declining in amplitude according to a mathematical law. I have further interposed between cerium and lead another half-swing of the pendulum. This renders the oscillations more symmetrical and brings gold, mercury, thallium, lead, and bismuth to the side where they are fully in harmony with members of previous groups.

The chemical elements are arranged in order, according to their atomic weights, on the centre vertical line which is divided into equal parts.

Following the curve from hydrogen downwards, we see that the elements forming the eighth group of Mendeleeff's arrangement are situate near three of the ten nodal points. This eighth group is divided into the three triplets—iron, nickel, and cobalt; rhodium, ruthenium, and palladium; iridium, osmium, and platinum.

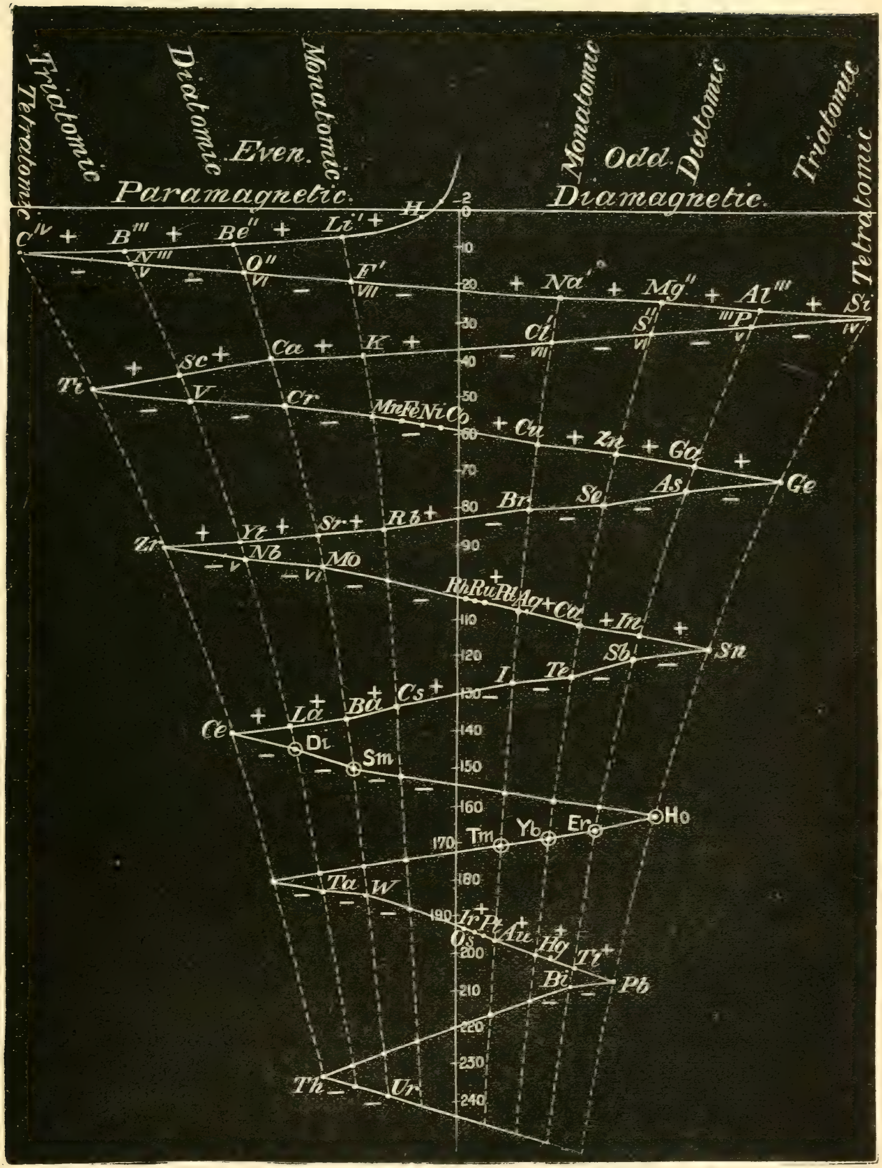
These bodies are interperiodic because their atomic weights exclude them from the small periods into which the other elements fall, and because their chemical relations with some members of the neighbouring groups show that they are probably interperiodic in the sense of being in transition stages.

Notice how accurately the series of like bodies fits into this scheme. Beginning at the top, run the eye down analogous positions in each oscillation, taking either the electro-positive or electro-negative swings. (See Table, p. 54.)

Notice, also, how orderly the metals discovered by spectrum analysis fit in their places—gallium, indium, and thallium; rubidium and cæsium.

The symmetry of nearly all this series proclaims at once that we are working in the right direction. Much also may be learned from the anomalies here visible. A few bodies, such as didymium, erbium,

Fig. 5.


Tetratomic
Triatomic
Diatomic
Monatomic
Even.
Paramagnetic.
Monatomic
Odd.
Diatomic
Triatomic
Tetratomic
Diamagnetic.
H
Li
Be
B
C
N
O
F
Na
Mg
Al
Si
P
S
Cl
K
Ca
Sc
Ti
V
Cr
Mn Fe Ni Co
Cu
Zn
Ga
Ge
As
Se
Br
Rb
Sr
Yt
Zr
Nb
Mo
Rh Ru Pd
Ag
Cd
In
Sn
Sb
Te
I
Cs
Ba
La
Ce
Di
Sm
Ho
Er
Yb
Tm
Ta
W
Ir Pt
Os
Au
Hg
Tl
Pb
Bi
Th
Ur

thulium, and ytterbium, are out of place, and require to have their atomic weights redetermined.

TABLE II.

VERTICAL SERIES OF THE ELEMENTS.

Even.							Odd.						
iv.	iii.	iii.	ii.	ii.	i.	i.	i.	i.	ii.	ii.	iii.	iii.	iv.
		v.		vi.		vii.	vii.		vi.		v.		
C	B	N	Be	O	Li	F	Cl	Na	S	Mg	P	Al	Si
Ti	Sc	V	Ca	Cr	K	Mn	Br	Cu	Se	Zn	As	Ga	Ge
Zr	Yt	Nb	Sr	Mo	Rb	—	I	Ag	Te	Cd	Sb	In	Sn
Ce	La	—	Ba	Sm	Cs	—	—	—	—	—	—	—	—
—	—	—	—	—	—	—	—	Au	—	Hg	Bi	Tl	Pb

The more I ponder over the arrangement of this zigzag curve, the more I become convinced that he who fully grasps its meaning holds the key to unlock some of the deepest mysteries of creation. As Mr. Browning puts it in his 'Parleyings'—"'Tis Man's to explore Up and down, inch by inch, with the taper his reason." Let us batter at the door of the Unknown and do our utmost to get a glimpse of some few of the secrets so darkly hidden.

Let us return in imagination to pre-geological ages, before the sun himself had been aggregated from the original protyle. We require two very reasonable postulates; let there be granted an antecedent form of energy having periodic cycles of ebb and swell, rest and activity. Let there also be granted an internal action akin to cooling operating slowly in the protyle. The first-born element would, in its simplicity, be most nearly allied to protyle. This is hydrogen, of all known bodies the simplest in structure and of the lowest atomic weight. For some time hydrogen would be the only existing form of matter (in our sense of the term). Between hydrogen and the next formed element there would be a gap in time, and in the interval the element standing next in order of simplicity would gradually be approaching its birth-point. In this interval we may suppose that the evolutionary process soon to determine the birth of a new element would fix likewise its atomic weight, its affinities, and its chemical position.

In this genesis of the elements the longer the time taken up in the cooling-down process, during which the hardening of protyle into atoms takes place, the more sharply defined would be the resulting elements; whilst the more rapid and the more irregular the cooling, the more closely the resulting bodies would fade into each other by almost imperceptible degrees. Thus we may conceive that the succession of events which gave rise to such groups as platinum, osmium, and iridium,—palladium, ruthenium, and rhodium,—iron, nickel, and cobalt,—might have produced only one element in each of these three groups if the process had been greatly prolonged. And conversely, had the rate of cooling been much more rapid, elements might have originated still more nearly identical than are

nickel and cobalt. Thus may have arisen the closely allied elements of the cerium, yttrium, and similar groups. In fact, we may regard the collocation of the minerals of the class of samarskite and gadolinite as a kind of cosmical lumber-room where elements in a state of arrested development—the unconnected missing links of inorganic Darwinism—are gathered together.

Any well-defined element may be likened to a platform of stability, connected by ladders of unstable bodies. In the first coalescence of the primitive stuff there would be formed the smallest atoms; these would then unite, forming larger groups; the gaps between the several stages would gradually be bridged over and the stable element appropriate to that stage would absorb, so to speak, the unstable rungs of the ladder which led up to it. It may be questioned whether there is an absolute uniformity in the mass of every ultimate atom even of one and the same chemical element. Probably our atomic weights merely represent a mean value around which the actual atomic weights of the atoms vary within certain narrow limits. When, therefore, we say that, e.g. the atomic weight of calcium is 40, the actual fact may well be, that whilst the majority of the calcium atoms really have the atomic weight of 40, some are represented by 39·9 or 40·1, a smaller number by 39·8 or 40·2, and so on. The properties which we perceive in any element are thus the mean of a number of atoms differing among themselves very slightly, but still not identical.* Is this the true meaning of Newton's "old worn particles?"

That this speculation, hazardous as it may seem, is in some respects supported by the experimental results above described will, I think, be admitted. It seems to me that the hypothesis I have just suggested, if taken in conjunction with the diagram, Fig. 5, enables us to proceed a step or two further along the track of the evolution of the elements. We may trace in the undulating curve the action of two forms of energy, the one acting vertically and the other vibrating to and fro like a pendulum. Let the vertical line represent temperature gradually sinking through an unknown number of degrees from the dissociation-point of the first-formed element downwards to the dissociation-point of the last member of the scale.

But what form of energy is figured by the oscillating line? We see it swinging to and fro to points equidistant from a neutral centre. We see this divergence from neutrality confer atomicity of one, two, three, or four degrees as the distance from the centre increases to one, two, three, or four divisions. We see the approach to or the retrocession from this same neutral line deciding the electro-negative

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* I venture to suggest that the heavier and lighter atoms formed from the protyle may have been partially sorted out by a process in nature somewhat analogous to the fractionation which has been already described. Such a sorting out would be effected chiefly whilst atomic matter was condensing from the primal state; but it may also have been carried on during geological ages in the wet way by successive solutions and re-precipitations of the various earths.

or electro-positive character of each element; those on the retreating half of the swing being positive, and those on the approaching half negative. In short, we are led to suspect that this oscillating power must be closely connected with the imponderable matter, essence, or source of energy we call electricity.

Let us now return to the period just preceding the birth of the first element. Before that time matter as it now is manifested did not exist. We can no more conceive of matter without energy than of energy without matter; indeed from one point of view the two are convertible terms. Let us assume that simultaneously with the creation of atoms all those attributes which enable us to discriminate one form of matter from another, start into being endowed with energy.

Our pendulum begins its swing from the electro-positive side; lithium, next to hydrogen in the simplicity of its atomic weight, is now formed, followed by glucinum, boron, and carbon. Each element, at the moment of birth, takes up definite quantities of electricity, and on these quantities its atomicity depends.* Thus are fixed the types of the monatomic, diatomic, triatomic, and tetratomic elements.

It has been pointed out by Dr. Carnelley that "those elements belonging to the even series of the periodic classification are always paramagnetic, whereas the elements belonging to the odd series are always diamagnetic." Now in our curve the even series to the left, so far as has been ascertained, are paramagnetic, whilst, with a few exceptions, all to the right are diamagnetic. The strongly magnetic group, iron, manganese, nickel, and cobalt, lie close together on the proper side. But the interperiodic groups, of which palladium and platinum are respectively examples, are supposed to be feebly magnetic. If this can be verified they form exceptions which have yet to be explained. Oxygen, which weight for weight is even more strongly magnetic than iron, lies near the beginning of the curve, whilst at the opposite end come the powerfully diamagnetic metals, bismuth and thallium.

We come now to the return or negative part of the swing; nitrogen appears and shows instructively how position governs the mean dominant atomicity. Nitrogen occupies a position immediately

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* "Nature presents us with a single definite quantity of electricity. . . . For each chemical bond which is ruptured within an electrolyte a certain quantity of electricity traverses the electrolyte, which is the same in all cases." —G. Johnstone Stoney, "On the Physical Units of Nature."—*British Association Meeting*, 1874, Section A. *Phil. Mag.*, May, 1881.

"The same definite quantity of either positive or negative electricity moves always with each univalent ion, or with every unit of affinity of a multivalent ion."—Helmholtz, Faraday Lecture, 1881.

"Every monad atom has associated with it a certain definite qnantity of electricity; every dyad has twice this quantity associated with it; every triad three times as much, and so on."—O. Lodge, "On Electrolysis," *British Association Report*, 1885.

below boron, a triatomic element, and, therefore, nitrogen is likewise triatomic. But nitrogen also follows upon carbon, a tetratomic body, and occupies the fifth position if we count from the place of origin. Now these seemingly opposing tendencies are beautifully harmonised by the endowment of nitrogen with a double atomicity, its atom being capable of acting either as a tri- or as a pentatomic element. With oxygen (di- and hexatomic) and fluorine (mon- and heptatomic) the same law holds good, and one half-oscillation of the pendulum is completed. Passing the neutral line again, we find successively formed the electro-positive bodies sodium (monatomic), magnesium (diatomic), aluminium (triatomic), and silicon (tetratomic).

Here we may notice a curious coincidence; at the beginning of this part of the curve stands carbon, the most ubiquitous element in the organic world. At the end, in opposition, stands silicon, the most commonly occurring element in the inorganic sphere. Further, as we move towards the median line, carbon is successively followed by nitrogen, oxygen, and fluorine, all entering into organic compounds and all gaseous in the free state. If we work back from silicon we find aluminium, magnesium, and sodium, all much less disposed to volatility, and all very prominent members of the mineral kingdom.

The first complete swing of the pendulum is accomplished by the birth of the three electro-negative elements, phosphorus, sulphur, and chlorine; all three, like the corresponding elements on the opposite homeward swing, having at least a double atomicity, depending on position.

Let us pause and examine the results. We have now formed the elements of water, of air, of ammonia, of carbonic acid, of plant and animal life; we have phosphorus for the brain, salt for the sea, clay and sand for the solid earth; two alkalies, an alkaline earth, an earth, along with their carbonates, borates, nitrates, fluorides, chlorides, sulphates, phosphates, and silicates, sufficient, it may be said, for animal and vegetable life, and for a world not so very different from that in which we live and move.

Again let us follow our pendulum. After the formation of chlorine this pendulum touches the neutral line, and is in the same position as in the beginning. Had everything remained as at first the next element to appear would again have been lithium, and the original cycle would have been eternally repeated, producing again and again the same fifteen elements. The conditions, however, are no longer the same: time has elapsed and the form of energy represented by the vertical line has declined; in other words, the temperature has sunk, and the first element to come into existence when the pendulum starts for its second oscillation is not lithium, but the metal next allied to it in the series, i. e. potassium, which may be regarded as the lineal descendant of lithium, with the same hereditary tendencies, but with less molecular mobility and a higher atomic weight.

Pass along the curve and in nearly every case the same law holds good. Thus the last element of the first complete vibration is chlorine. In the corresponding place in the second vibration we have not an exact repetition of chlorine but the very similar body bromine, and when the same position recurs for the third time we see iodine. I need not multiply examples. I may, however, point out that we have here a phenomenon which reminds us of alternating or cyclical generation in the organic world, or we may perhaps say of atavism, a recurrence to ancestral types, somewhat modified.

In this evolutionary scheme it cannot be expected that the potential elements should all be equal to each other. On the contrary, many degrees of stability will be represented, and if we look with a scrutinising eye we shall see our old friend the "missing link," coarse enough to be detected in the groups comprising such bodies as iron, nickel, and cobalt; palladium, ruthenium, and rhodium; iridium, osmium, and platinum: whilst in a more subtile form these missing links present themselves as representatives of the differences which I have suggested between the atoms of the same chemical element.

On the even or paramagnetic half of the swing the energy appears to have acted in a very irregular manner, whilst on the odd, or diamagnetic half, there is considerable regularity. Thus, between the extreme odd elements, silicon (28), germanium (73), tin (118), a missing element (163), and lead (208) there is a difference of exactly 45 units, rendering this half of the curve remarkably symmetrical. On the even side the differences are 36, 42, 51, 39 and 53 (assuming an atomic weight of 180 for a missing element between cerium and thorium). At first sight these differences appear to follow no law, but they gain interest when we see that the mean differences of these figures is 44·2—almost exactly the same as that on the odd side of the curve.

From this uniformity of difference—actual on the one side and average on the other—we may fairly infer that whilst on the odd side there has been little or no variation in the force symbolised by the vertical line, minor irregularities have been the rule on the even side. Or, in other words, the fall of temperature has been very uniform on the odd side—where, accordingly, we see that every original element represents a well marked group, sodium, magnesium, aluminium, silicon, phosphorus, and chlorine; whilst on the even side the temperature has fallen with considerable fluctuations, thus preventing the formation here of any well-marked groups of elements, excepting those of which lithium and glucinum are the leading types.

Having thus detected irregularities in the fall of temperature in the protyle, we may next ask is there any fluctuation in the force represented by the pendulum-movement? This movement I have assumed to be connected with electrical energy. The earliest-formed elements are those in which chemical energy is at a maximum; as

we descend the scale the affinities become feebler and the chemism grows more and more sluggish. In part this change may be due to the circumstance that the elements generated at a reduced temperature no longer possess great molecular mobility. But it is also extremely probable that the chemism-forming energy is itself dying out like the fires of the cosmic furnace. I have attempted to symbolise this gradual fading by a decrease in the amplitude of vibration.

The figures representing the scale of atomic weights may be supposed to represent, inversely, the scale of a gigantic pyrometer plunged into a cauldron where the elements of suns and worlds are undergoing formation. As the heat sinks, the elements generated increase in density and atomic weight. Below the formation-point of uranium the temperature will probably permit of the earlier-born elements forming combinations among themselves, and we shall witness, e. g. the birth of water, and the formation of those known compounds the dissociation of which is not beyond the powers of our terrestrial sources of heat.

Turning to the upper portion of the diagram we see that there is little room for elements of a lower atomic weight than hydrogen. But let us pass "through the looking-glass" and cross the zero line. What shall we find on the other side? Dr. Carnelley asks for an element of negative atomic weight; and here is ample room and verge enough for a shadow series of such unsubstantialities, leading, perhaps, to that "Unseen Universe" which two eminent physicists have discussed. Helmholtz says that electricity is probably as atomic as matter;* is electricity one of the negative elements? and the luminiferous ether another? Matter, as we now know it, does not here exist; and the forms of energy which are apparent in the motions of matter are as yet only latent possibilities.

A genesis of the elements such as is here sketched would not be confined to our little solar system, but would probably follow the same general sequence of events in every centre of energy now visible as a star.

It may be said that so far I have proved nothing. But I may submit that at least I have shown the improbability of the persistence of the ultimate character, and the eternal self-existence, the fortuitous origin, and the simultaneous creation of the elements. The analogy of these elements with the organic radicles, and still more with living organisms, constrains us to suspect that they are compound bodies, springing from a process of evolution. We have drawn corroborative evidence from the distribution and the association of the rare earths, evidence which seems to be converging to the

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* "If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions, which behave like atoms of electricity."—HELMHOLTZ, Faraday Lecture, 1881.

point of assuming a direct character. Led by the great law of continuity I have ventured to suggest a process by which our elements may have been originated. I dare not say *must* have been originated, for no one can be better aware than I am how much remains to be done before this great, this fundamental question can be finally solved. I earnestly hope that others will take up the task, and that chemistry, like biology, may find its Darwin.

If we consider the position we occupy with reference to the primary questions of chemistry, we might compare research to a game of chess. Man, the investigator, is playing, not with Satan for his soul, but with Nature for knowledge and power. Each element has its allotted moves on the great board of the universe; some of them dependent solely on themselves, and others on the interaction of the adjacent elements. Some of our elements may be compared to pawns, others to knights, bishops, or castles. The game is fearfully unequal. Our antagonist knows the power and the limitations of every piece, all the laws of the game, all possible moves, and is merciless in exacting penalty for errors. We experimentalists know nothing but what we have learned in countless losing games. But our knowledge is increasing. Nature no longer gives us fool's mate. The struggle becomes more obstinate, more exciting, we come upon new gambits, new combinations, and though still checkmated at the last, we take a few pawns, perhaps even a piece or two. Such partial successes were achieved when Lavoisier introduced the use of the balance and developed the theory of combustion; when Dalton put forward the atomic theory; when Davy decomposed the alkalies; when Wöhler effected the synthesis of urea; and when Faraday first liquefied a gas. On such and many similar occasions I can imagine our antagonist becoming thoughtful.

But suppose we one day win the game; that we find out what these obstinate elements really are, that we learn how they came into being, and wherefore their number, their properties, and their mutual relations are such as we find them? We shall then know, *à priori*, what we have now to find out by special experiment; we shall foresee the results of every conceivable reaction, and our theories will legitimate themselves by the power of prediction. To attain such knowledge seems to me the grand task of the chemistry of the coming age.

If you think I have given too free rein to the "scientific imagination" you will, I hope, forgive me as one who at least does not despair of the future of our Science.

[W. C.]

WEEKLY EVENING MEETING,

Friday, February 25, 1887.

WILLIAM HUGGINS, Esq. D.C.L. LL.D. F.R.S. Manager and Vice-President, in the Chair.

CAPTAIN W. DE W. ABNEY, R.E. F.R.S. *M.R.I.*

Sunlight Colours.

SUNLIGHT is so intimately woven up with our physical enjoyment of life that it is perhaps not the most uninteresting subject that can be chosen for what is—perhaps somewhat pedantically—termed a Friday evening "discourse." Now, no discourse ought to be possible without a text on which to hang one's words, and I think I found a suitable one when walking with an artist friend from South Kensington Museum the other day. The sun appeared like a red disk through one of those fogs which the east wind had brought, and I happened to point it out to him. He looked, and said, "Why is it that the sun appears so red?" Being near the railway station, whither he was bound, I had no time to enter into the subject, but said if he would come to the Royal Institution this evening I would endeavour to explain the matter. I am going to redeem that promise, and to devote at all events a portion of the time allotted to me in answering the question why the sun appears red in a fog. I must first of all appeal to what every one who frequents this theatre is so accustomed to, viz. the spectrum; I am going not to put it in the large and splendid stripe of the most gorgeous colours before you with which you are so well acquainted, but my spectrum will take a more modest form of purer colours some twelve inches in length.

I would ask you to notice which colour is most luminous. I think that no one will dispute that in the yellow we have the most intense luminosity, and that it fades gradually in the red on the one side and in the violet on the other. This then may be called a qualitative estimate of relative brightnesses; but I wish now to introduce to you what was novel last year, a quantitative method of measuring the brightness of any part.

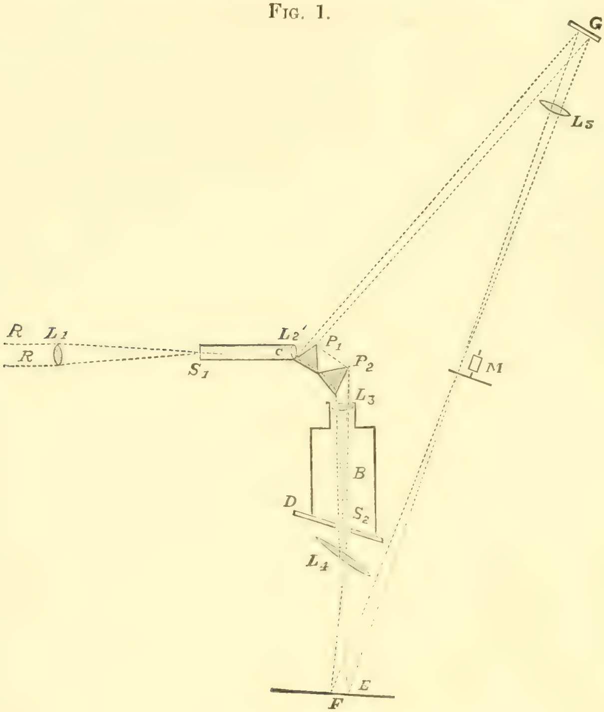
Before doing this I must show you the diagram of the apparatus which I shall employ in some of my experiments.

R R are rays (Fig. 1) coming from the arc light, or, if we were using sunlight, from a heliostat, and a solar image is formed by a lens, L_1, on the slit S_1 of the collimator C. The parallel rays produced by the lens L_2 are partially refracted and partially reflected. The former pass through the prisms P_1P_2, and are focused to form a spectrum by

a lens, L_3, on D, a movable ground-glass screen. The rays are collected by a lens, L_4, tilted at an angle as shown, to form a white image of the near surface of the second prism on F.

Passing a card with a narrow slit S_2, cut in it in front of the spectrum, any colour which I may require can be isolated. The consequence is that, instead of the white patch upon the screen, I

FIG. 1.



Colour Photometer.

have a coloured patch, the colour of which I can alter to any hue lying between the red and the violet. Thus, then, we are able to get a real patch of very appropriately homogeneous light to work with, and it is with these patches of colour that I shall have to deal. Is there any way of measuring the brightness of these patches? was a question asked by General Festing and myself. After trying various plans, we hit upon the method I shall now show you, and if any one works with it he must become fascinated with it on account of its almost childish simplicity—a simplicity, I may remark, which it took us some months to find out. Placing a rod before the screen, it casts a black shadow surrounded with a coloured background. Now I may

cast another shadow from a candle or an incandescence lamp, and the two shadows are illuminated, one by the light of the coloured patch and the other by the light from an incandescence lamp which I am using to-night. [Shown.] Now one stripe is evidently too dark. By an arrangement which I have of altering the resistance interposed between the battery and the lamp, I can diminish or increase the light from the lamp, first making the shadow it illuminates too light and then too dark compared with the other shadow which is illuminated by the coloured light. Evidently there is some position in which the shadows are equally luminous. When that point is reached, I can read off the current which is passing through the lamp, and having previously standardised it for each increment of current, I know what amount of light is given out. This value of the incandescence lamp I can use as an ordinate to a curve, the scale number which marks the position of the colour in the spectrum being the abscissa. This can be done for each part of the spectrum, and so a complete curve can be constructed which we call the illumination curve of the spectrum of the light under consideration.

Now, when we are working in the laboratory with a steady light, we may be at ease with this method, but when we come to working with light such as the sun, in which there may be constant variation owing to passing, and maybe usually imperceptible, mist, we are met with a difficulty; and in order to avoid this, General Festing and myself substituted another method, which I will now show you. We made the comparison light part of the light we were measuring. Light which enters the collimating lens partly passes through the prisms and is partly reflected from the first surface of the prism; that we utilise, thus giving a second shadow. The reflected rays from P_1 fall on G, a silver-on-glass mirror. They are collected by L_5, and form a white image of the prism also at F. The method we can adopt of altering the intensity of the comparison light is by means of rotating sectors, which can be opened or closed at will, and the two shadows thus made equally luminous. [Shown.] But although this is an excellent plan for some purposes, we have found it better to adopt a different method. You will recollect that the brightest part of the spectrum is in the yellow, and that it falls off in brightness on each side, so, instead of opening and closing the sectors, they are set at fixed intervals, and the slit is moved in front of the spectrum, just making the shadow cast by the reflected beam too dark or too light, and oscillating between the two till equality is discovered. The scale number is then noted, and the curve constructed as before. It must be remembered that, on each side of the yellow, equality can be established.

This method of securing a comparison light is very much better for sun work than any other, as any variation in the light whose spectrum is to be measured affects the comparison light in the same degree. Thus, suppose I interpose an artificial cloud before the slit of the spectroscope, having adjusted the two shadows, it will be seen

that the passage of steam in front of the slit does not alter the relative intensities; but this result must be received with caution. [The lecturer then proceeded to point out the contrast colours that the shadow of the rod illuminated by white light assumed.]

I must now make a digression. It must not be assumed that every one has the same sense of colour, otherwise there would be no colour-blindness. Part of the researches of General Festing and myself have been on the subject of colour-blindness, and these I must briefly refer to. We test all who come by making them match the luminosity of colours with white light, as I have now shown you; and as a colour-blind person has only two fundamental colour perceptions instead of three, his matching of luminosities is even more accurate than is that made by those whose eyes are normal or nearly normal. It is curious to note how many people are more or less deficient in colour-perception. Some have remarked that it is impossible that they were colour-blind, and would not believe it, and sometimes we have been staggered at first with the remarkable manner in which they recognised colour to which they ultimately proved deficient in perception. For instance, one gentleman when I asked him the name of a red colour patch, said it was sunset colour; he then named green and blue correctly, but when I reverted to the red patch he said green. On testing further he proved totally deficient in the colour-perception of red, and with a brilliant red patch he matched almost a black shadow. The diagram shows you the relative perceptions in the spectrum of this gentleman and myself. There are others who only see three-quarters, others half, and others a quarter the amount of red that we see, whilst some see none. Others see less green and others less violet, but I have met with no one that can see more than myself or General Festing, whose colour-perceptions are almost identical. Hence we have called our curve of illumination the "normal curve."

We have tested several eminent artists in this manner, and about one-half of the number have been proved to see only three-quarters of the amount of red which we see. It might be thought that this would vitiate their powers of matching colour, but it is not so. They paint what they see, and although they see less red in a subject, they see the same deficiency in their pigments; hence they are correct. If totally deficient, the case of course would be different.

Let us carry our experiments a step further, and see what effect what is known as a turbid medium has upon the illuminating value of different parts of the spectrum. I have here water which has been rendered turbid in a very simple manner. In it has been very cautiously dropped an alcoholic solution of mastic. Now mastic is practically insoluble in water, and directly the alcoholic solution comes in contact with the water it separates out in very fine particles, which, from their very fineness, remain suspended in the water. I propose now to make an experiment with this turbid water.

I place a glass cell containing water in front of the slit, and on

the screen I throw a patch of blue light. I now change it for turbid water in a cell. This thickness much dims the blue; with a still greater thickness the blue has almost gone. If I measure the intensity of the light at each operation, I shall find that it diminishes according to a certain law, which is of the same nature as the law of absorption. For instance, if one inch diminishes the light one-half, the next will diminish it half of that again, the next half of that again, whilst the fourth inch will cause a final diminution of the total light of one-sixteenth. If the first inch allows only one-quarter of the light, the next will only allow one-sixteenth, and the fourth inch will only permit 1/256 part to pass. Let us, however, take a red patch of light and examine it in the same way. We shall find that, when the greater thickness of the turbid medium we used when examining the blue patch of light is placed in front of the slit, much more of this light is allowed to pass than of the blue. If we measure the light we shall find that the same law holds good as before, but that the proportion which passes is invariably greater with the red than the blue. The question then presents itself: Is there any connection between the amounts of the red and the blue which pass? Lord Rayleigh, some years ago, made a theoretical investigation of the subject; but, as far as I am aware no definite experimental proof of the truth of the theory was made till it was tested last year by General Festing and myself. His law was that for any ray, and through the same thickness, the light transmitted varied inversely as the fourth power of the wave-length. The wave-length 6000 lies in the red, and the wave-length 4000 in the violet. Now 6000 is to 4000 as 3 to 2, and the fourth powers of these wave-lengths are as 81 to 16, or as about 5 to 1. If, then, the four inches of our turbid medium allowed three-quarters of this particular red ray to be transmitted, they would only allow $(\frac{3}{4})^5$, or rather less than one-fourth, of the blue ray to pass. Now this law is not like the law of absorption for ordinary absorbing media, such as coloured glass for instance, because here we have an increased loss of light running from the red to the blue, and it matters not how the medium is made turbid, whether by varnish, suspended sulphur, or what not. It holds in every case, so long as the particles which make the medium turbid are small enough; and please to recollect that it matters not in the least whether the medium which is rendered turbid is solid, liquid, or air. Sulphur is yellow in mass, and mastic varnish is nearly white, whilst tobacco-smoke when condensed is black, and very minute particles of water are colourless: it matters not what the colour is, the loss of light is *always* the same. The result is simply due to the scattering of light by fine particles, such particles being small in dimensions compared with a wave of light. Now, in this trough is suspended 1/1000 of a cubic inch of mastic varnish, and the water in it measures about 100 cubic inches, or is 100,000 times more in bulk than the varnish. Under a microscope of ordinary power it is impossible to distinguish any particles of varnish: it looks like a

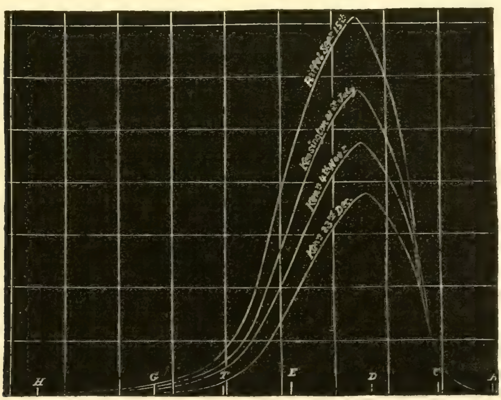
homogeneous fluid, though we know that mastic will not dissolve in water. Now a wave-length in the red is about 1/40,000 of an inch, and a little calculation will show that these particles are well within the necessary limits. Prof. Tyndall has delighted audiences here with an exposition of the effect of the scattering of light by small particles in the formation of artificial skies, and it would be superfluous for me to enter more into that. Suffice it to say that when particles are small enough to form the artificial blue sky they are fully small enough to obey the above law, and that even larger particles will suffice. We may sum up by saying that very fine particles scatter more blue light than red light, and that consequently more red light than blue light passes through a turbid medium, and that the rays obey the law prescribed by theory. I will exemplify this once more by using the whole spectrum and placing this cell, which contains hyposulphite of soda in solution in water, in front of the slit. By dropping in hydrochloric acid, the sulphur separates out in minute particles; and you will see that, as the particles increase in number, the violet, blue, green, and yellow disappear one by one and only red is left, and finally the red disappears itself.

Now let me revert to the question why the sun is red at sunset. Those who are lovers of landscape will have often seen on some bright summer's day that the most beautiful effects are those in which the distance is almost of a match to the sky. Distant hills which when viewed close to are green or brown, when seen some five or ten miles away appear of a delicate and delicious, almost of a cobalt, blue colour. Now, what is the cause of this change in colour? It is simply that we have a sky formed between us and the distant ranges, the mere outline of which looms through it. The shadows are softened so as almost to leave no trace, and we have what artists call an atmospheric effect. If we go into another climate, such as Egypt or amongst the high Alps, we usually lose this effect. Distant mountains stand out crisp with black shadows, and the want of atmosphere is much felt. [Photographs showing these differences were shown.] Let us ask to what this is due. In such climates as England there is always a certain amount of moisture present in the atmosphere, and this moisture may be present as very minute particles of water—so minute indeed that they will not sink down in an atmosphere of normal density—or as vapour. When present as vapour the air is much more transparent, and it is a common expression to use, that when distant hills look "so close" rain may be expected shortly to follow, since the water is present in a state to precipitate in larger particles; but when present as small particles of water the hills look very distant, owing to what we may call the haze between us and them. In recent weeks every one has been able to see very multiplied effects of such haze. The ends of long streets, for instance, have been scarcely visible though the sun may have been shining, and at night the long vistas of gas lamps have shown light having an increasing redness as they became more distant. Every one admits the

presence of mist on these occasions, and this mist must be merely a collection of intangible and very minute particles of suspended water. In a distant landscape we have simply the same or a smaller quantity of street-mists occupying, instead of perhaps 1000 yards, ten times that distance. Now I would ask, What effect would such a mist have upon the light of the sun which shone through it?

It is not in the bounds of present possibility to get outside our atmosphere and measure by the plan I have described to you the different illuminating values of the different rays, but this we can do:—First, we can measure these values at different altitudes of the sun, and this means measuring the effect on each ray after passing through different thicknesses of the atmosphere, either at different times of day, or at different times of the year, about the same hour. Second, by taking the instrument up to some such elevation as that to which Langley took his bolometer at Mount Whitney, and so to leave the densest part of the atmosphere below us. Now, I have adopted both these plans. For more than a year I have taken measurements of sunlight in my laboratory at South Kensington, and I have also taken the instrument up to 8000 feet high in the Alps, and made observations *there*, and with a result which is satisfactory in that both sets of observations show that the law which holds with artificially turbid media is under ordinary circumstances obeyed by sunlight in passing through our air: which is, you will remember, that more of the red is transmitted than of the violet, the amount of each depending on the wave-length. The luminosity of the spectrum observed

FIG. 2.



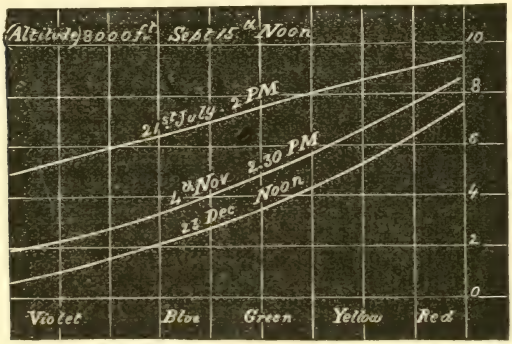
Relative Luminosities.

at the Riffel I have used as my standard luminosity, and compared all others with it. The result for four days you see in the diagram.

I have diagrammatically shown the amount of different colours which penetrated on the same days, taking the Riffel as ten. It will be seen that on December 23 we have really very little violet and less than half the green, although we have four-fifths of the red.

The next diagram before you shows the minimum loss of light which I have observed for different air thicknesses. On the top we

FIG. 3.



Proportions of Transmitted Colours.

have the calculated intensities of the different rays outside our atmosphere. Thus we have that through one atmosphere, and two, three, and four; and you will see what enormous absorption there is in the blue end at four atmospheres. The areas of these curves, which give the total luminosity of the light, are 761, 662, 577, 503, and 439; and if observed as astronomers observe the absorption of light, by means of stellar observations, they would have had the values, 761, 664, 578, 504, and 439—a very close approximation one to the other.

Next notice in the diagram that the top of the curve gradually inclines to go to the red end of the spectrum as you get the light transmitted through more and more air, and I should like to show you that this is the case in a laboratory experiment. Taking a slide with a wide and long slot in it, a portion is occupied by a right-angled prism, one of the angles of 45° being towards the centre of the slot. By sliding this prism in front of the spectrum I can deflect outwards any portion of the spectrum I like, and by a mirror can reflect it through a second lens, forming a patch of light on the screen overlapping the patch of light formed by the undeflected rays. If the two patches be exactly equal, white light is formed. Now, by placing a rod as before in front of the patch, I have two coloured stripes in a white field, and though the background remains of the same intensity of white, the intensities of the two stripes can be altered by moving

the right-angled prism through the spectrum. The two stripes are now apparently equally luminous, and I see the point of equality is where the edge of the right-angled prism is in the green. Placing a narrow cell filled with our turbid medium in front of the slit, I find that the equality is disturbed, and I have to allow more of the yellow to come into the patch formed by the blue end of the spectrum, and consequently less of it in the red end. I again establish equality. Placing a thicker cell in front, equality is again disturbed, and I have to have less yellow still in the red half, and more in the blue half. I now remove the cell, and the inequality of luminosity is still more glaring. This shows, then, that the rays of maximum luminosity must travel towards the red as the thickness of the turbid medium is increased.

The observations at 8000 feet, here recorded, were taken on September 15 at noon, and of course in latitude 46° the sun could not be overhead, but had to traverse what would be almost exactly equivalent to the atmosphere at sea-level. It is much nearer the calculated intensity for no atmosphere intervening, than it is for one atmosphere. The explanation of this is easy. The air is denser at sea-level than at 8000 feet up, and the lower stratum is more likely to hold small water particles or dust in suspension than is the higher.

For, however small the particles may be, they will have a greater tendency to sink in a rare air than in a denser one, and less water vapour can be held per cubic foot. Looking, then, from my laboratory at South Kensington, we have to look through a proportionately larger quantity of suspended particles than we have at a high altitude when the air thicknesses are the same; and consequently the absorption is proportionately greater at sea-level than at 8000 feet high. This leads us to the fact that the real intensity of illumination of the different rays outside the atmosphere is greater than it is calculated from observations near sea-level. Prof. Langley, in this theatre, in a remarkable and interesting lecture, in which he described his journey up Mount Whitney to about 12,000 feet, told us that the sun was really blue outside our atmosphere, and at first blush the amount of extra blue which he deduced to be present in it would, he thought, make it so; but though he surmised the result from experiments made with rotating disks of coloured paper, he did not, I think, try the method of using pure colours, and consequently, I believe, slightly exaggerated the blueness which would result. I have taken Prof. Langley's calculations of the increase of intensity for the different rays, which I may say do not quite agree with mine, and I have prepared a mask which I can place in the spectrum giving the different proportions of each ray as calculated by him, and this when placed in front of the spectrum will show you that the real colour of sunlight outside the atmosphere, as calculated by Langley, can scarcely be called bluish. Alongside I place a patch of light which is very closely the colour of sunlight on a July day at noon in England. This comparison will enable you to gauge the blueness,

and you will see that it is not very blue, and, in fact, not bluer perceptibly than that we have at the Riffel, the colour of the sunlight at which place I show in a similar way. I have also prepared some screens to show you the value of sunlight after passing through five and ten atmospheres. On an ordinary clear day you will see what a yellowness there is in the colour. It seems that after a certain amount of blue is present in white light the addition of more makes but little difference in the tint. But these last patches show that the light which passes through the atmosphere when it is feebly charged with particles does not induce the red of the sun as seen through a fog. It only requires more suspended particles in any thickness to induce it.

In observations made at the Riffel, and at 14,000 feet, I have found that it is possible to see far into the ultra-violet, and to distinguish and measure lines in the sun's spectrum which can ordinarily only be seen by the aid of a fluorescent eye-piece or by means of photography. Circumstantial evidence tends to show that the burning of the skin, which always takes place in these high altitudes in sunlight, is due to the great increase in the ultra-violet rays. It may be remarked that the same kind of burning is effected by the electric arc light, which is known to be very rich in these rays.

Again, to use a homely phrase, "You cannot eat your cake and have it." You cannot have a large quantity of blue rays present in your direct sunlight, and have a luminous blue sky. The latter must always be light scattered from the former. Now, in the high Alps you have, on a clear day, a deep blue-black sky, very different indeed from the blue sky of Italy or of England ; and as it is the sky which is the chief agent in lighting up the shadows, not only in those regions do we have dark shadows on account of no intervening—what I will call—mist, but because the sky itself is so little luminous. In an artistic point of view this is important. The warmth of an English landscape in sunlight is due to the highest lights being yellowish, and to the shadows being bluish from the sky-light illuminating them. In the high Alps the high lights are colder, being bluer, and the shadows are dark, and chiefly illuminated by reflected direct sunlight. Those who have travelled abroad will know what the effect is. A painting in the Alps, at any high elevation, is rarely pleasing, although it may be true to Nature. It looks cold, and somewhat harsh and blue.

In London we are often favoured with easterly winds, and these, unpleasant in other ways, are also destructive of that portion of the sunlight which is the most chemically active on living organisms. The sunlight composition of a July day may, by the prevalence of an easterly wind, be reduced to that of a November day, as I have proved by actual measurement. In this case it is not the water particles which act as scatterers, but the carbon particles from the smoke.

Knowing, then, the cause of the change in the colour of sunlight, we can make an artificial sunset, in which we have an imitation light

passing through increasing thicknesses of air largely charged with water particles. [The image of a circular diaphragm placed in front of the electric light was thrown on the screen in imitation of the sun, and a cell containing hyposulphite of soda placed in the beam. Hydrochloric acid was then added: as the fine particles of sulphur were formed, the disk of light assumed a yellow tint, and as the decomposition of the hyposulphite progressed, it assumed an orange and finally a deep red tint.] With this experiment I terminate my lecture, hoping that in some degree I have answered the question I propounded at the outset: why the sun is red when seen through a fog.

[W. de W. A.]

WEEKLY EVENING MEETING,

Friday, March 4, 1887.

SIR WILLIAM BOWMAN, Bart. LL.D. F.R.S. Manager and Vice-President, in the Chair.

VICTOR HORSLEY, Esq. F.R.S. B.S. F.R.C.S.

Brain Surgery in the Stone Ages.

THE title of this discourse fairly expressed its scope, for the practice by the people of the neolithic period of resorting to surgery for the relief of mischief to the brain was fully detailed.

It seemed scarcely necessary to do more than briefly refer to the gradual advance of civilisation through the stages of stone, bronze, and iron; but it was pointed out that while the Greeks at the time of their siege of Troy had only half learnt how to use iron, the northern nations of Europe were still in the stone period. Neither did it appear necessary to do more than briefly allude to the general habits of neolithic people, save to draw attention to the fact, that whatever be its explanation, the instances in which trephining was practised in the stone age occur more frequently in the centre of France than anywhere else in Europe. The deliberate nature of the operation, as exemplified in the skulls hitherto discovered, was proved by the position of the openings, their being in the majority of instances healed, and by the extremely interesting discovery of the fact that the portions of bone cut out were not only preserved as *amulets*, but also put back again into such a trephined head at the time of death. From a comparison of the modes of trephining performed by savage and mediæval nations, it was proved that the stone age people opened the skull either by drilling, scraping, or sawing, most probably by the last method. Similarly it was shown from a study of the seat of operation, that in all probability recourse to surgery was suggested by the symptoms of depressed fracture, and notably by the symptoms of traumatic epilepsy.

GENERAL MONTHLY MEETING,

Monday, March 7, 1887.

Henry Pollock, Esq. Treasurer and Vice-President, in the Chair.

Gerrard Ansdell, Esq. F.C.S.
Alexander Thomas Binny, Esq.
Mrs. Amelia Coles,
Eugene H. Cowles, Esq.
Samuel Fenwick, M.D.
Edwin Freshfield, Esq. LL.D. V.P.S.A.
Edward Kraftmeier, Esq.
Thomas Hayter Lewis, Esq. F.S.A.
Captain Andrew Noble, C.B. F.R.S.
Charles M. Roupell, Esq. M.A.
Major Ricarde Seaver,
Sir Henry Mervyn Vavasour, Bart.
Major-General Edmond Walker, R.E.

were elected Members of the Royal Institution.

A Letter was read to His Grace The President from Sir Frederick Abel, Organising Secretary to the Imperial Institute, dated February 4, 1887, requesting the co-operation of the Royal Institution in the promotion of the establishment of the Imperial Institute.

The Chairman described the course which the Managers propose to adopt in effecting this co-operation, and stated that Subscriptions for the Imperial Institute would be received at the Royal Institution, and a List of such Subscriptions suspended in the Hall.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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WEEKLY EVENING MEETING,

Friday, March 11, 1887.

Henry Pollock, Esq., Treasurer and Vice-President, in the Chair.

The Ven. Archdeacon Farrar, M.A. D.D. F.R.S.

Society in the Fourth Century after Christ.

After a brief sketch of the fourth century in its varied and striking elements of life, and the great leaders of religious thought whom it produced, the lecturer proceeded to mention some of its broad general characteristics.

1. It was an age of great calamities, in which men suffered from war, pestilence, famine, and the incursions of barbarians. The sack of Rome affected the civilised world like the shock of an earthquake. The news of it fell with stunning force on the mind of St. Jerome, in his cave at Bethlehem, and if St. Augustine was less deeply moved by it, and felt chiefly concerned to prove that the catastrophe was not the consequence of Christianity, the reason was that he was chiefly interested in the City of God, while the City of Evil was left to its predestined ruin.

2. The highest no less than the lowest orders of society were affected by these calamities. The age was full of "strange stories of the deaths of kings." There was scarcely a single Emperor or usurper of the century who did not perish by murder, suicide, or execution, and the presentiment of Julian was justified, who, on first putting on the Imperial purple, exclaimed,

> τὸν δὲ κατ' ὄσσε
> ἔλλαβε πορφύρεος θάνατος καὶ μοῖρα κραταίη.

Or, as Chapman renders it,

> "Death with his purple finger shut, and violent fate his eyes."

3. Yet the Emperors were surrounded with that elaborate pomp and circumstance which we call Byzantinism. Constantine's cruelty, and his passion for jewels, suggested the stinging epigram,

> "Saturni aurea sæcla quis requirat?
> Sunt hæc gemmea, sed Neroniana!"

The court was filled with pompous ceremonies and slavish officialism. The title "Your Eternity" was just beginning to come into fashion, when it was scoffed out of court by St. Athanasius. By way of illustration, a description was given of one of the splendid summer processes of the Emperor Arcadius. Yet the Emperors were often slaves to their own eunuchs.

4. This age of calamity was also an age of boundless luxury, of

which details were given from the Homilies of St. Chrysostom, and the History of the Pagan soldier Ammianus Marcellinus.

5. The luxury flaunted itself side by side with the most grinding poverty. The general misery of the tax-crushed multitude was illustrated by anecdotes from Palladius, St. Chrysostom, St. Basil, and St. Ambrose.

6. The fourth century was also an age of struggling beliefs, in which many guided their professions exclusively by considerations of social and political expediency.

7. The combination of circumstances thus illustrated led to an ever deepening superstition both among Pagans and Christians. The Pagan spirit of superstition was illustrated by stories from the sophist Eunapius, and by the wild outburst of panic against the use of sorcery in the days of Valens—a panic which closely resembled the frantic state of suspicion prevalent in the days of the Popish plot. The superstitious spirit of Christians was evinced by the adoration paid to very dubious relics, and by the credulous acceptance of the most portentous miracles, as shown in the biographies of St. Martin of Tours, St. Gregory Thaumaturgus, and others.

8. Yet, amid all these superstitions and catastrophes, the quiet everyday beautiful life of humanity was going on, as was shown by peaceful pictures of the sweet Christian homes in which men like St. Gregory of Nazianzus, St. Basil, and St. Chrysostom in the East, and St. Ambrose in the West, were trained to usefulness and self-devotion.

9. The lecturer proceeded to furnish various illustrations of the life of the young. He gave anecdotes of the boyhood of St. Augustine; of the pranks played by students in the days of St. Gregory of Nazianzus in the University of Athens; of the rite of mock initiation known as "the Bath"; of the Professor Proaeresius and the boy-student Eunapius.

10. Some account was also given of the varied experiences of St. Augustine as a Professor of Rhetoric at Carthage, Milan, and Rome; of the turbulent young students known as the *Eversores* in Africa; and of the beautiful assiduity and skill with which Augustine trained his young pupils, Licentius and Trygetius, in his retreat at Cassiciacum. The lecturer gave some curious and striking anecdotes of the life of a youthful law student at Rome, as illustrated in the adventures of Alypius, a friend of St. Augustine, who was afterwards baptised with him.

11. Details were then furnished of the life of eminent heathen sophists, and eminent Christian bishops; and it was shown from Ammianus Marcellinus, as well as from Christian writers, that pomp and prosperity exercised a very unfortunate influence on the lives and characters of many bishops in the great cities.

12. The raging party spirit and riotous slander which prevailed in this century were shown by circumstances in the lives of the great Fathers, and especially of St. Athanasius. The lecturer

strongly enforced the lesson, taught by these narratives, of the duty of fairness and kindness in judging alike of ancient and contemporary characters.

13. The passion for theological disputation which prevailed in this century was illustrated by remarkable passages from St. Jerome and St. Augustine.

14. The lecturer then proceeded to deal with the subject of Asceticism, its extraordinary growth and strange developments, together with the disastrous effects which it produced on many minds. This part of the subject was chiefly illustrated by details derived from St. Chrysostom, St. Jerome, and Cassianus; while from anecdotes of St. Antony (whom the lecturer, however, regarded as a purely legendary person) and St. Macarius, it was shown that even the most rigid hermits were sometimes ready to acknowledge that the ordinary course of Christian virtue might be a more excellent way than their own.

In the epilogue to the lecture it was pointed out that there were many points of resemblance between the fourth century and our own, and some remarks were made on the elements which saved the civilisation of the fourth century from total destruction, and which may still be fruitful in blessings to mankind.

WEEKLY EVENING MEETING,

Friday, March 18, 1887.

Sir William Bowman, Bart. LL.D. F.R.S. Manager and Vice-President in the Chair.

George John Romanes, Esq. M.A. LL.D. F.R.S. *M.R.I.*

*Mental Differences between Men and Women.**

After quoting sundry representative opinions upon the subject, from Aristotle downwards, the lecturer proceeded to enumerate what appeared to him the leading features of the distinction when men and women were received as classes or *en masse.* The inferiority of the female mind was displayed most conspicuously in a comparative *absence of originality*, especially in the higher levels of intellectual work. In her powers of acquisition the woman stood nearer to the man, although even here she was behind him; for, as soon as the age of adolescence was reached, there was a greater power of amassing knowledge on the part of the male. As musical executants, however, and also as writers of fiction, equality could be fairly asserted.

With regard to *judgment*, the female mind was apt to take superficial views, to be unduly biased from the side of the emotions, and in general to display comparative weakness. On the other hand, their greater refinement of nervous organisation led to more delicate powers of sensuous perception and rapidity of thought on the part of women.

In this connexion Mr. Romanes gave the results of experiments which he had conducted on rapidity of reading, whereby it was shown that, as a rule, women could read much faster than men. Passing on to the *emotions*, he remarked that in women these were almost always less under control of the will than in men, being usually more volatile and displayed a greater tendency to childishness—the petty forms of resentment which belonged to a shrew or a scold, caprice, vanity, fondness of display, of social excitement, being all more characteristic of the feminine than of the masculine temperament.

On the other hand, the meritorious qualities wherein the female mind stood pre-eminent were affection, sympathy, devotion, modesty, long-suffering, reverence, religious feeling, and in general the gentler virtues as distinguished from the heroic. Therefore, when a woman performed an act of heroism, the prompting motives were almost sure to be of an unselfish kind. Hence, also, it was women who first flocked in numbers to the standard of the Cross, and became followers of the religion which, by changing the whole ideal of ethics —or assigning the highest place to the gentler and domestic

* A full report of the discourse is published in the 'Nineteenth Century' for May 1887.

virtues—was destined afterwards so greatly to exalt in the estimation of man the character which belonged by nature to woman.

Dealing lastly with the *will*, Mr. Romanes observed that this was certainly less powerful in women than in men. We rarely found in the former that firm tenacity of purpose and determination to overcome all obstacles which was characteristic of what we called a manly mind; and when a woman was urged to any prolonged exercise of volition, the prompting cause was usually to be found in the emotional side of her nature. Moreover, even in the lesser displays of volitional activity required for close reading, studious thought, &c., women were usually less able to concentrate their attention; and therefore they seldom specialised their pursuits to the extent usual among men. Their indecision of character often led to timidity and diffidence in adopting any line of conduct where issues of importance were concerned, thus leaving them in the painful condition, as they graphically expressed it, of "not knowing their own minds."

Coming next to the causes of these mental differences between the sexes, the lecturer argued that the biological principles of selection had determined the physical superiority of male animals in general, and with it the psychological qualities of courage, self-reliance, determination, and, in short, all those mental characters which belonged to a consciousness of bodily strength; while, conversely, members of the opposite sex had acquired the opposite characters. And in the case of our own species these principles of selection further operated with a conscious reference to psychical as well as to physical endowments, thus acting directly as well as indirectly in severing the psychological characters of sex.

Again, the maternal instincts and the prolonged association of the mother with her children in our own species imparted to her a fulness of emotional life, the whole quality of which was distinctively feminine. Thus, in accordance with the law of inheritance as limited by sex, we could understand how these influences became in successive generations cumulative; while in the fondness of little girls for dolls we might note an interesting example in psychology of the law of inheritance at earlier periods of life.

There remained but one other assignable cause of the mental differences in question. This cause was *education.* From the condition of abject slavery to which woman was consigned in the lower levels of human evolution to her condition at the present time, her mental culture had been widely different from that of man. It was not until the middle of the present century that any attempt was anywhere made to provide for the higher education of women. But now, whether we liked it or not, the woman's movement was upon us, and we must endeavour to guide the flood into the most beneficial channels. What were these channels? Assuredly not those which ran straight athwart all the mental differences between men and women. No amount of female education could ever make equal this natural inequality, nor was it desirable that it should. Woman was the

natural complement, not the natural rival of man; and the qualities of mind wherein he excels were not, *sui generis*, the most exalted of human faculties. Mere strength, whether of mind or of body, was not the highest criterion of nobility; the truest grandeur of human nature was revealed by that nature as a whole, and here there could be no doubt that the feminine type was fully equal to the masculine. But while we might hope that social opinion might ever continue to oppose the woman's movement in its most extreme forms—or those forms which aimed at setting up an unnatural and therefore impossible rivalry in the struggles of practical life—we might also hope that social opinion would soon become unanimous in its encouragement of the higher education of women.

The lecturer proceeded to enumerate the many advantages to which this would lead, and to dispose of arguments on the other side.

[G. J. R.]

WEEKLY EVENING MEETING,

Friday, March 25, 1887.

Sir Frederick Abel, C.B. D.C.L. F.R.S. Manager and Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*

On the Colours of Thin Plates.

The physical theory, as founded by Young and perfected by his successors, shows how to ascertain the composition of the light reflected from a plate of given material and thickness when the incident light is white; but it does not, and cannot tell us, except very roughly, what the *colour* of the light of such composition will be. For this purpose we must call to our aid the theory of compound colours, and such investigations as were made by Maxwell upon the chromatic relations of the spectrum colours themselves. Maxwell found that on Newton's chromatic diagram the curve representative of the spectrum takes approximately the simple form of two sides of a triangle, of which the angular points represent a definite red, a definite green, and a definite violet. The statement implies that yellow is a compound colour, a mixture of red and green.

In illustration of this fact, an experiment was shown in which a compound yellow was produced by absorbing agents. An infusion of litmus absorbs the yellow and orange rays; a thin layer of bichromate of potash removes the blue. Under the joint operation of these colouring matters the spectrum is reduced to its red and green elements, as may be proved by prismatic analysis; but, if the proportions are suitably chosen, the colour of the mixed light is yellow or orange. When the slit of the usual arrangement is replaced by a moderately large circular aperture, the prism throws upon the screen two circles of red and green light, which partially overlap. Where the lights are separated, the red and green appear; where they are combined, the resultant colour is yellow.

On the basis of Maxwell's data it is possible to calculate the colours of thin plates and to exhibit the results in the form of a curve upon Newton's diagram. The curve starts at a definite point, corresponding to an infinitely small thickness of the plate. This point is somewhat upon the blue side of white. As the thickness increases the curve passes very close to white, a little upon the green side. It then approaches the side of the triangle, indicating a full orange; and so on. In this way the colours of the various orders of Newton's scale are exhibited and explained. The principal dis-

crepancy between the curve and the descriptions of previous observers relates to the precedence of the reds of the first and second orders. The latter has usually been considered to be the superior, while the diagram supports the claim of the former. The explanation is to be found in the inferior brightness (as distinguished from purity) of the red of the first order and its consequent greater liability to suffer by contamination with white light. Such white light, foreign to the true phenemenon, is always present when the thin plate is a plate of air enclosed between glass lenses. To make the comparison fairly, a soap film must be used, or recourse may be had to the almost identical series of colours presented by moderately thin plates of doubly refracting crystals when traversed by polarised light. Under these circumstances the red of the first order is seen to be equal or superior to that of the second order.

[Rayleigh.]

WEEKLY EVENING MEETING,

Friday, April 1, 1887.

Sir Frederick Abel, C.B. D.C.L. F.R.S. Manager and Vice-President, in the Chair.

Professor Dewar, M.A. F.R.S. *M.R.I.*

Light as an Analytic Agent.

Analytical research by means of the agency of light is usually carried out, by observing the effects produced on light of known quality by transmission through or reflection from matter; or on the other hand, by stimulating matter by the agency of heat or electricity to evolve light, and thereby noting its special qualities.

In previous lectures I have given an account of the results of a series of experiments, undertaken in concert with my colleague Prof. Liveing, with the object of elucidating certain obscure spectroscopic phenomena, and I propose this evening to extend the record of our work.

Not long since Berthelot published the results of some investigations of the rate of propagation of the explosion of mixtures of oxygen with hydrogen and other gases. He found that in a mixture of oxygen and hydrogen in the proportions in which they occur in water, the explosion progressed along a tube at the rate of 2841 metres per second; not far from the velocity of mean square for hydrogen particles, on the dynamic theory of gases, at a temperature of 2000°.

This is a velocity which, though very far short of the velocity of light, bears a ratio to it which cannot be called insensible. It is in fact about $\frac{1}{105000}$ part of it. Hence if the explosion were advancing towards the eye, the waves of light would proceed from a series of particles lit up in succession at this rate. This would be equivalent to a shortening of the wave-length of the light by about $\frac{1}{105000}$ part; and in the case of the yellow sodium lines would produce a shift of the lines towards the more refrangible side of the spectrum by a distance of about $\frac{1}{107}$ of the space between the two lines. It would require an instrument of very high dispersive power and sharply defined lines to make such a displacement appreciable. With lines of longer wave-length than the yellow sodium lines, the displacement would be proportionately greater. Further, if a receding explosion could be observed simultaneously with an advancing explosion, the relative shift of the line would be doubled, one image of the line observed being thrown as much towards the less-refrangible side of its proper position as the other was thrown towards the more-refrangible side. The two images of the red line of lithium would in

this way be separated by a distance of about $\frac{1}{8}$ of a unit of Angström's scale; a quantity quite appreciable, though much less than the distance between the components of b_3, and about equal to the distance of the components of the less refrangible of the pair of lines E. We thought therefore that we might test theory by experiment.

A preliminary question had, however, to be answered. What lines could be seen in the flash of the exploding gases? We were pretty certain that the hydrogen lines could not be seen, but that probably we might get sufficient dust of sodium compounds floating in the gas to make the sodium lines visible. A preliminary observation was made on the flash of mixed hydrogen and oxygen in a Cavendish's eudiometer, which showed not only the yellow sodium lines, but the orange and green bands of lime and the indigo line of calcium all very brightly, as well as other lines not identified. The flash is very instantaneous, but nevertheless produces a strong impression on the eye; and by admitting the light of a flame into the spectroscope at the same time as that of the flash, the identity of the lines was established. That sodium should make itself seen was not surprising, but that the spectrum of lime should also be so bright had not been anticipated. At first we thought that some spray of the water over which the gases were confined must have found its way into the eudiometer; but subsequent observations led us rather to suppose that the lime was derived from the glass of the eudiometer. The lime-spectrum made its appearance when the eudiometer was quite clean and dry, and when the gases had been standing over water for a long time.

To obtain the high dispersion requisite, as already explained, we made use of one of Rowland's magnificent gratings, with a ruled surface of $3\frac{1}{8}$ by $2\frac{1}{8}$ inches, and the lines 14,438 to the inch. One telescope fitted with a collimating eye-piece served both as collimator and observing-telescope; and by this means we were able to use the spectra of the third and fourth orders with good effect.

Observations were made with this instrument on explosions in an iron tube shown in section in Fig. 1, half an inch in diameter, fitted at the end with a thick glass plate (a), held on by a screw-cap (c) and made tight with leaden washers. Small lateral tubes (d, d), at right angles to the main tube, were brazed into it near the two ends, for the purpose of connecting it with the air-pump, admitting the gases, and firing them. For this last purpose a platinum wire (b) fused into glass was cemented into the small tube, so that an electric spark could be passed from the wire to the side of the small tube when the gases were to be exploded.

To bring out the lithium lines, a small quantity of lithium carbonate in fine powder was blown into the tube before the cap with glass plate was screwed on. Powder was used because we supposed that it must be loose dust which would be lighted up by the explosion. The lithium lines came out bright enough, and it was unnecessary to put in any more lithium for any number of explosions. The tube

was of course quite wet after the first explosion from the water formed, but the lithium lines were none the less strong. Indeed, after the tube had been very thoroughly washed out, the lithium lines continued to be visible at each explosion, though less brightly than at first. A good deal of continuous spectrum accompanies the flash which, from the overlapping of spectra of different orders, makes observations difficult, so a screen of red glass was used to cut this off when the lithium red line was under observation. In any case, however, close observation of the flashes is very trying, from the suddenness with which the illumination appears, and the briefness of its duration. At first we compared the lithium line given by the flash of the exploding gases with that produced by the flame of a small Bunsen burner in which a bead of fused lithium carbonate was held, both being in the field at once. While the flame-line was sharply defined, the flash-line had a different character, and was always diffuse at the edges; so that it was not possible in this way to substantiate the minute difference of wave-length indicated by theory, though the flash-line certainly seemed a little the more refrangible of the two.

We then tried taking the explosion in a tube bent round so as to be returned upon itself, the two parts of the tube being parallel to each other, and the glass ends side by side (Fig. 2). The axis of the

fig.1.

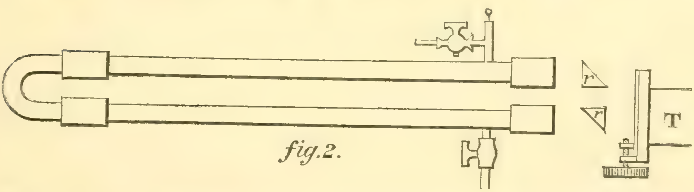

fig.2.

collimator (T) was made to coincide with that of one limb of the tube, so that the flash in that limb was seen directly; and by means of two reflecting prisms (r, r) the light from the other limb was thrown into the slit, and the two images were seen simultaneously one above the other. As the gas was ignited from one end of the tube, the flash was seen receding in one limb, approaching in the other, so that the displacement of the two lines would be doubled. Still we were unable to substantiate any relative displacement of the lines on account of their breadth and diffuse character. By washing out the tube the breadth of the lines was considerably reduced, but they remained diffuse at the edges, and baffled any observation sufficiently accurate to establish a displacement. Certainly there appeared to be

a very slight displacement, but it was not so definite that one could be sure of it.

These observations, however, led us to some other interesting results. In the first place, one of the two images of the lithium line almost always was reversed—that is, showed a dark line down the middle. This was the line given by the flash approaching the slit. The receding flash in the other limb of the tube gave as broad a bright line, but it had no dark line in its middle. This observation was made a great many times; and the fact of the reversal established independently in the case of some other metallic lines by means of photographs. These reversals show that in the wave of explosion the temperature of the gases does not reach its maximum all at once; but the front of the wave is cooler than the part which follows and absorbs some of its radiation, while the rear of the wave does not produce the same effect. One would suppose that there must be cooler lithium-vapour in the rear of the wave as well as in its front; but it is possible that the absorption produced by it extends over the whole width of the line, and not only over a narrow strip in the middle. For we observed that when a little lithium carbonate was freshly put into the tube, the red line was so much expanded as to fill the whole field of view—that is to say, it was some ten or twelve times as wide as the distance between the two yellow lines of sodium; but by washing out the tube with water (that is, by reducing the quantity of lithium present in the tube), the line could be reduced in width until it was no wider than one-tenth of the distance between the two sodium lines. This seems to prove that the breadth of the line is directly dependent on the amount of lithium present.

M. Fievez has, in a recent publication ('Bulletins de l'Académie royale de Belgique'), concluded, from observations on sodium, that the widening of the lines is solely due to elevation of temperature. The flash of the exploding gases cannot be raised in temperature by the presence of a minute quantity more of a lithium compound; so that in our case the widening cannot be ascribed to anything but the increase in the quantity of lithium present, or to some consequence of that increase. It is not improbable that the amount of lithium vaporised in the front of the wave of explosion is less than in the following part, and hence the absorption-line is not so wide as the bright line behind it, while in the rear of the wave the absorption extends over the whole width of the bright band, and so is not so easily noticed. Only twice amongst many observations was any reversal of the lithium line seen in the receding wave of explosion.

On observing the flash with a spectroscope of small dispersion instead of that with the grating, the continuous spectrum was very bright, but the metallic lines stood out still brighter; not only the red line of lithium, but the orange, the green, and the blue lines were very bright, and continued so when the pressure of the gases before explosion was reduced from one atmosphere to one-third of an atmosphere. The violet line was not seen, but it may have been so

much expanded as to be lost in the continuous spectrum; for it showed in a photograph afterwards taken. Other lines were, however, seen—the sodium yellow lines, the calcium indigo line, a group of other blue lines, and a group of green lines, amongst which one line was conspicuous, and this line, by comparison with the solar spectrum, was identified with E. We had not expected to see any lines of iron, as iron and its compounds give no lines in the flame of a Bunsen burner, and we supposed that it would only be volatilised at a much higher temperature. But the appearance of E suggested that other of the green and blue lines might be due to iron; so we proceeded to compare the positions of these lines with those of the electric spark between iron electrodes. For this purpose one of the spark-lines was first brought carefully on to the pointer, or cross wires, in the eye-piece of the observing telescope, and then, the passage of the spark being stopped, the flash of the exploding gases was observed. It was not difficult to see whether any line was on the pointer; and the observation could be repeated as many times as was desired without any shift of the apparatus. Nine of the most conspicuous green and yellowish-green lines in the flash were thus identified with lines of iron. For the blue and violet we adopted the photographic method as much less trying to the eyesight. Eight to twelve flashes were taken in succession without any shift of the apparatus, so as to accumulate their effects on the photographic plate. Eight flashes were found enough in general to produce a good impression, and more than twelve could not well be taken without turning out the water which accumulated in the tube, and cleaning the glass which closed its end. After the flashes had been taken, but without shifting the photographic plate, the slit of the spectroscope was partly covered, and the electric spark between iron points passed in front of the slit. We had thus on the plate the photograph of the flash as well as of the spark. Fourteen more lines in the indigo and violet were thus identified with iron lines; and on extending the photographs into the ultra-violet, and substituting quartz lenses and prisms for the glass ones hitherto used, a much larger number of lines were identified. There could be no doubt, then, that we had iron vapour in the flash. We supposed that it must be derived from dust of oxide shaken by the explosion off the sides of the tube, and we had the tube bored out clean and bright like a gun-barrel. This made no diminution in the brightness or number of the lines; and we came to the conclusion that the explosion detached particles of iron from the tube, and converted them into vapour. This was confirmed by finding that, however carefully the tube had been cleaned, the glass ends always became clouded with a rusty deposit after ten or twelve flashes. Altogether 68 lines of iron have been identified in the flash, of which about 40 lie in the ultra-violet between H and O. Only one iron line above O has been definitely identified, and that in only a few photographs. It is T.

As iron gave so many lines in the flash it was reasonable to sup-

pose that more volatile metals would give their lines too. Linings of thin sheet copper, lead, cadmium, zinc, aluminium, and tin were successively put into the tube, and their effects on the flash observed. Copper gave one strong line in the green (wave-length 5104·9), but no other line in the visible part of the spectrum. In the ultra-violet two strong lines between Q and R came out in the photographs, frequently as reversed lines. Some of the photographs showed also something of the shaded bands in the blue which are ascribed to the oxide of copper. The green line of copper had been observed in the flash before the copper lining was put into the tube; and we concluded that the copper was derived from the brass with which the small lateral tubes were fastened into the large tube, or that the iron of the tube contained a little copper. When the leaden lining was used, only one visible line of lead was developed, and that was the strong violet line, but two ultra-violet lines between M and N were strongly depicted on the photographic plates. The violet line of lead had also been observed in many of the photographs taken before the leaden lining was introduced. This we ascribed to the leaden washers used to make the glass or quartz plates air-tight. The line was greatly increased in strength by the leaden lining. The zinc lining gave no visible line at all, notwithstanding the easy volatility of the metal; and in the ultra-violet it gave only a very doubtful impression of one of the lines near P. The cadmium, aluminium, and tin linings gave no lines at all. Zinc dust put into the tube gave no zinc lines, merely increased the continuous spectrum, and speedily rendered the quartz end opaque.

A clean wire of magnesium put into the tube gave the b group of lines, but no other line. No trace of the blue line, so conspicuous in the flame of burning magnesium, nor of the triplets near L and S, nor of the very strong line, the strongest of all in the arc, at wave-length 2852. b_1 and b_2 were well seen; but as b_4 is an iron line, as well as a magnesium line, and the iron line was visible in the flash before the magnesium wire was introduced, we cannot be sure whether the magnesium line, as well as the iron line, was present in the flash. Magnesia did not develop any line at all; merely augmented the continuous spectrum.

Compounds of sodium, such as the carbonate and chloride, introduced in powder gave the ultra-violet line between P and Q strongly, frequently reversed; but no other line except of course D. Potassium compounds developed, often reversed, the pair of violet lines, and also the ultra-violet pair near O, but no others.

A strip of silver developed two ultra-violet lines, one on either side of P; but we could not detect in the flash the well-known green lines of that metal. When powder of silver oxalate was introduced, the yellowish-green line (w.l. 5464) was seen at the first explosion but not afterwards. As silver oxalate is itself an explosive compound, decomposing with an evolution of heat, it is reasonable to ascribe the appearance of this line at the first explosion to the extra temperature so engendered.

Strips of copper, electroplated with nickel, brought out almost all the strong nickel lines in the ultra-violet between K and Q; 25 were photographed. When nickel oxalate was put in so as to give a powder of metallic nickel after the first explosion, the same lines were developed, and three additional lines in the ultra-violet. Only one line was seen in the visible part of the spectrum, and that was the yellowish-green line (w.l. 5476).

Copper wires electroplated with cobalt gave in the flash 22 lines in the violet and ultra-violet, between G and P; no lines beyond those limits. Cobalt oxalate gave no more.

No other metal gave anything like so many lines as iron, nickel, and cobalt; and it is remarkable that almost all the lines of these metals developed in the flash lie in the same region between G and P.

We expected that manganese would have given several lines in the flash; but it was not so. Neither metallic manganese, nor any of several compounds which we tried, gave us any lines of that metal except the violet triplet, and this was generally given by the iron tube alone, and was merely stronger for the manganese put in. The green channellings characteristic of manganese, and ascribed to the oxide, were, however, well seen when metallic manganese was used.

Chromium, introduced as bichromate of ammonia, which of course became chromium oxide at the first flash, gave three triplets in the green, the indigo, and the ultra-violet near N respectively, but no other lines.

Bismuth, antimony, and arsenic gave no lines, nor did mercury spread over a sheet of copper lining the tube. Several metals were tried as amalgams spread over such a piece of copper, but with no fresh results, except in the case of thallium, which gave the green line strongly, the strong line between L and M, and two lines between N and O.

On the whole it does not appear that the form in which the metal is introduced into the tube makes much difference. The merest traces of those which gave lines were sufficient. Generally when a metal had been put into the tube, its lines continued to show after the strip or lining had been removed. Thus, after the nickel strips had been taken out, and the tube cleaned out as completely as it could be mechanically, the nickel lines still came out in the flash, and the same was the case with other metals.

The strongest part of the water-spectrum, from *s* to near R, generally impressed itself more or less on the photographic plate; but, with the exception of T, which was only developed once or twice, no lines made their appearance in the region more refrangible than *s*.

Thus far the experiments had been made with the gases at the atmospheric pressure, or nearly so, before ignition. The proportions of hydrogen and oxygen were nearly two to one; but an excess of either gas to the extent of one-fifth did not sensibly affect the results.

Other explosive mixtures were tried. Carbonic oxide with

oxygen, and marsh-gas with oxygen, developed in general the same lines as the hydrogen mixture, but gave a much brighter continuous spectrum. Sulphuretted hydrogen, arseniuretted hydrogen, and antimoniuretted hydrogen, exploded with oxygen, also gave very bright continuous spectra, but no lines attributable to sulphur, arsenic, or antimony.

We have also tried explosions at higher pressures; mixtures of hydrogen, carbonic oxide, and marsh-gas respectively, with oxygen, were compressed into the tube by a condensing syringe until the pressure reached two and a half atmospheres, and in some cases three and a half atmospheres. The general effect of increasing the pressure was to strengthen very much the continuous spectrum, and also to intensify the bright lines, so that photographs could be taken with a smaller number of explosions. The lines previously observed to be reversed were more strongly reversed, but no new lines which we can attribute to the metals employed were noticed. No iron line more refrangible than T showed itself in the photographs. But a banded spectrum, of which traces had been noticed in the flash of the gases at lower pressure, came out decidedly. This spectrum occupies the region between P and R; it is not a regularly channelled spectrum, though probably under higher dispersion it would resolve itself into groups of lines like the water-spectrum. In fact it seems to us most probable that it is a development of the water spectrum, dependent on the pressure.

It seems very remarkable that metals so little volatile as iron, nickel, and cobalt should develop so many lines* in the flash, while more volatile metals show few or no lines. We do not know that any lines attributed to the metals, as distinct from their compounds, which have been observed in the gas-flame cannot be seen also in the flash of the exploding gases, unless they be the blue lines of zinc which Lecoq de Boisbaudran has seen faintly in the gas-flame when zinc chloride was introduced. These are, however, so faint in the flame, that they might easily escape notice in the much stronger continuous spectrum of the flash. But iron, nickel, and cobalt show no lines of those metals in a gas-flame. Mitscherlich (' Ann. de Phys. u. Chem.' Bd. 121, St. 3), by mixing vapour of ferric chloride with the hydrogen burnt in an oxyhydrogen-jet, obtained a number of the lines of iron. These form three groups—one below D, one near E, and one near G. The last two groups have a general correspondence with the lines developed in the explosions in the visible part of the spectrum; but exact identification is not possible with his figure. Of other metals he seems also to have found the same lines in the oxyhydrogen-jet which we have seen in the explosions, but with additional lines in several cases. Thus he found three zinc and as many cadmium lines, two of mercury, four of copper, and so on.

Gouy (' Comptes Rendus,' lxxxiv. 1877, p. 232) has observed in

* For detailed list of these lines see ' Proc. Roy. Soc.' vol. xxxvi. pp. 473–5.

the inner green cone of a modified Bunsen burner fed with gas mixed with spray of iron-salts, four green lines of iron which we did not find in the flash. He saw two of the blue lines, but not the other lines which we have noticed. In like manner with cobalt, he observed two feeble blue rays which we did not see in the explosions; also one zinc, one cadmium, and one silver line which we did not see; and he did not notice the green copper line which we always have seen in the explosions. In other cases he has noticed the same lines that we have noticed.

Comparing the spectrum of the explosions with that of iron wire burnt in a jet of coal-gas fed with oxygen, they may be called identical. We find in them generally the same lines and the same relative strengths of the lines. For instance, in the explosion-spectrum the strength of the groups of lines on either side of M and the line at wave-length 3859·2 is decidedly greater as compared with the other lines than it is in the arc-spectrum of iron. It is the same in that of iron burnt as above mentioned. T, however, comes out more strongly in the last-mentioned spectrum than in the explosions.

German-silver wire burnt in the coal-gas and oxygen jet gave the same nickel and copper lines as were developed in the explosions. Silver wire gave in the same jet the two silver lines near P, but no channelled spectrum. Spray of cobalt chloride gave also the same lines as in the explosions, with a few additional; while spray of manganese chloride gave the strong manganese triplet at wave-length about 2800, more refrangible than anything observed in the explosions, besides the usual violet triplet.

On the whole the spectra produced by the jet of coal-gas and oxygen are very similar to those of the explosions as far as the metallic lines go; they exhibit a few more lines, or it may be these are more easily observed.

Of the green and blue lines of iron seen by us in explosions nine are registered by Watts as occurring in the flame of a Bessemer converter; or at least the lines he gives are so near that we cannot doubt their identity.

When we come to make a comparison with the spectrum of the spark-discharge from a solution of ferric chloride, the differences become more marked. Not only are there many more lines in the spark-spectrum, but the relative intensities of those lines which are common to both spark and explosion are very different, and two of the iron lines seen in the explosions appear to be absent from the spark. The differences between the spectrum of the spark taken from a liquid electrode and that given by solid electrodes has usually been attributed to the lower temperature of the former; but the absence from the former spectrum of the line at wave-length 4132, and the feebleness of the line at wave-length 4143, both strong lines in the arc and in the explosions, as well as in the spark between solid electrodes, seem to indicate that the differences of spark-spectra

are not simply due to differences of temperature. In fact we know so little about the mechanism, so to speak, of the changes of electric energy into heat, and of heat into radiation, that there is no good reason for assuming that the energy which takes the form of radiation in the electric discharge through a gas must first take the form of the motion of translation of the particles on which temperature depends. The gas may, for a short time, be intensely luminous at a very low temperature; and if the impulses which give rise to the vibratory movements of the particles be of different characters, the characters of the vibrations also may differ within certain limits.

Leaving, however, the realms of speculation, we may mention that we have before observed the spectrum of iron at a temperature intermediate between that of the oxyhydrogen-jet and that of the electric arc.

Some time since ('Proc. R. S.' xxxiv. p. 119, and 'Proc. Camb. Phil. Soc.' iv. p. 256) we described the spectrum proceeding from the interior of a carbon tube strongly heated by the electric arc playing on the outside. This spectrum approaches more nearly to that of the arc inasmuch as it shows all, or almost all, the iron lines given by the arc between F and O, and the aluminium pair between H and K; but it resembles the explosion-spectrum in the relative strength of some of the iron lines, and in the absence of almost all iron lines between O and T. The iron lines seen reversed against the hot walls of the carbon-tube correspond with the strongest of the explosion-lines; the strong lines near M and a little below L in the explosions being those most strongly reversed in the photographs of the carbon-tube. The greater completeness and extent of the iron spectrum, as well as the presence of the aluminium lines, which are entirely wanting in the explosion-spectrum, indicate that the temperature of the tube was higher than that of the explosion. That iron, nickel, and cobalt are volatile in some degree at the temperature of the explosion appears to be proved, and makes the appearance of iron lines at the very apices of solar prominences, as observed by Young, less astounding than it seemed to be at first sight. The ascending current of gas making the prominence may very well carry iron vapour with it; or we may not unreasonably suppose that there is meteoric dust containing iron everywhere in the outer atmosphere of the sun, which becomes volatilised, and emits the radiation observed, when it is heated up by the hot current of the prominence. What the temperature of such a current may be we cannot well gauge, but it is high enough to give the hydrogen-spectrum, of which no trace has been observed in the flash of the explosions or in the oxy-hydrogen-jet. The temperature of the explosions we know with tolerable accuracy, at least when the gases are at atmospheric pressure to begin with. Bunsen ('Phil. Mag.' 1867, p. 494) found the pressure of the explosion was for hydrogen and oxygen 9·6 atmospheres, and for carbonic oxide and oxygen 10·3 atmospheres, and he calculated the corresponding temperatures to be 2844° and 3033°.

Recently published observations by Berthelot and Vieille ('Comptes Rendus,' xcviii. 1884, p. 548) put the pressure of explosion of oxygen and hydrogen at 9·8 atmospheres, and of carbonic oxide and oxygen at 10·1, and the corresponding temperatures 3240° and 3334°. The pressures determined by the two observers agree closely, and the calculated temperatures are not very discordant. On the whole, we cannot be wrong in assuming the temperature of the exploding gases to be about 3000°; and we see that at this degree such metals as iron, nickel, and cobalt are vaporous, and emit many characteristic rays, and that by far the greatest part of these rays lie between narrow limits of refrangibility, G and P. Even for other metals there is a predominance of rays in the same part of the spectrum. The lines of lead, potassium, and manganese, three out of four lines of thallium, and two-thirds of those of chromium, observed in the explosions, fall within the same region. It must not be inferred that these facts indicate the limit of the rate of oscillation which can be set up in consequence of an elevation of temperature to 3000°, because we know that the spectrum of the lime-light extends much further. But it might be possible to establish a sort of spectroscopic scale of temperatures if the lines which are successively developed as the temperature rises were carefully noted. Thus the appearance of the iron line T seems to synchronise with temperature of about 3000°. The lithium blue line is invisible in the flame of an ordinary Bunsen burner, but is just visible at the temperature of the inner green cone formed by reducing the proportion of gas to air in such a burner, while in the exploding gas the green line too is seen. It seems to need a temperature above 3000° to get the aluminium lines at H. Probably no line is ever abruptly brought out at a particular temperature—it will always be gradually developed as the temperature rises; yet the development may be rapid enough to give an indication which may be useful in default of means of more exact measurement. In former papers treating of spectroscopic problems ('Proc. Roy. Soc.' vol. xxxiv. p. 130, and xxix. p. 489) we have more than once adverted to the necessity of the study of the spectra both of flames and of the electric discharge under modified conditions of pressure. The projected experiments on the arc in lime-crucibles have not yet been carried out; but the present is a first instalment of a study of flame-spectra under such conditions.

[J. D.]

GENERAL MONTHLY MEETING,

Monday, April 4, 1887.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. LL.D. President, in the Chair.

The Managers reported, That at their Meeting on the 7th of March last the following letter from Dr. Tyndall to the Honorary Secretary was read :—

HIND HEAD, HASLEMERE.
March 6th, 1887.

MY DEAR SIR FREDERICK BRAMWELL,

The year's holiday so graciously and considerately granted me by the Managers will come to an end next month ; and it therefore behoves me to state without further delay, for the information of the Managers, how matters stand with me.

A brief conversation with my friend Sir Frederick Pollock, and my own reflections thereupon, have convinced me that, instead of making a statement myself at the Board Meeting on Monday, it will be more expedient to embody what I have to say in a letter to you.

For more than one-third of a century it has been my privilege to enjoy the unfailing sympathy and encouragement of the Managers and Members of the Royal Institution. It is now my duty to return to their hands the trust which they first committed to me in the spring of 1853. I have come to this resolution on account of the need I feel of thorough rest, and of freedom from engagements, as to lecturing, the non-fulfilment of which would be detrimental to the Institution, and a cause of sore distress to myself.

Worries connected with building, and other worries inimical to quietude of brain, have for the last few years troubled me much. These are now, for the most part, things of the past, so that the freedom I seek will, I doubt not, soon restore me to good health.

I returned from Switzerland in October so refreshed and invigorated that I hoped to be able to cope successfully with all the duties then before me. I had assured myself of the friendly aid of Mr. Crookes, and had even arranged to go to Paris to purchase some instruments necessary for my contemplated work. To the end of the year my health continued strong. Then came a long-continued spell of withering easterly winds, which chilled me, dried me up, and brought on an attack of sleeplessness, intense while it lasted, but which, happily, has in great part disappeared with its cause.

Of my ultimate and complete recovery I entertain little doubt. Still it would be obviously unfair to the Members, as it would be intolerable to myself, to allow the fortunes of our great Institution to depend in any degree upon such caprices of health. It is therefore my desire to make room for a successor whose years and vigour will place him beyond all changes and chances of this kind.

Of the feelings called forth by my separation from the Royal Institution I have said nothing. But the Managers will understand that my silence in this respect is due, not to the absence of such feelings, but only to the conviction that on the present occasion the less said about them the better.

Believe me,
Most faithfully yours,
JOHN TYNDALL.

The Managers further reported, That at their adjourned Meeting on the 21st of March the following Resolutions were adopted :—

Resolved, "That the Managers desire to record by this Resolution the expression of their deep regret that the state of Dr. Tyndall's health should have rendered necessary the resignation of his position of Professor of Natural Philosophy at the Royal Institution, and that it should have compelled the Managers to accept that resignation—and desire at the same time to record the expression of their hope that the relief thus obtained from the arduous duties of the Professorship will conduce to his speedy and complete recovery.

"The Managers also desire that there should be recorded the expression of their thorough appreciation of the unremitting and most valuable services which during the long period of thirty-four years Dr. Tyndall has rendered to the Royal Institution in carrying out the duties of his office—services which not only have upheld and have advanced the position of the Royal Institution, but have benefited science and the world at large."

"The Managers having ascertained without doubt that Professor Tyndall altogether declines to receive any pension or pecuniary testimonial in recognition of his services to the Royal Institution, and that in parting from his long connection with it he desires only to carry with him the friendly recollection and goodwill of the Members,"

Resolved, "That this generous and disinterested refusal to accept any acknowledgment of a pecuniary nature upon the occasion of his retirement cannot fail to increase the feelings of regard and respect which must be entertained for Professor Tyndall's devotion of so much of his life to the important researches which have been prosecuted by him in the laboratories of the Institution, and for the delivery of those eloquent lectures in its theatre which has done so much to support its scientific renown and to increase its prosperity. The Managers, therefore, under the before-mentioned circumstances of Dr. Tyndall's refusal to accept any pension or pecuniary testimonial, resolved that some marked recognition of a permanent character should be given to the opinion entertained of the great value of Professor Tyndall's labours now about to cease, and the Managers trust that it may prove as agreeable to him as it will be honourable to the Institution, if he would kindly comply with a request which they recommend should be made to him, to sit for his bust (in marble), to be placed in the house of the Institution in perpetual memory of his relations with it, and the cost of which should be defrayed by a vote from the general funds of the Institution, and in order further to perpetuate and honour the name of Professor Tyndall in connection with the Institution, the Managers recommend that one of the courses of Lectures delivered annually in the Institution shall be called the Tyndall Lectures."

Resolved, "That the Honorary Secretary, in informing Dr. Tyndall of the acceptance of his resignation, do send to him a copy of the foregoing Resolutions."

The Managers further reported, That at their Meeting held this day the following letter was read :

Dear Sir Frederick Bramwell, Hind Head, *3rd April*, 1887.

I have halted in my reply to your letter of March 23rd, through sheer inability to express the feeling which the action of the Managers, at their meeting on the 21st, has called into life.

And my reply must now be brief; for I hardly dare trust myself to dwell upon the "Resolutions" which you have conveyed to me. Taken in connexion with the severance of my life from the Royal Institution, and with the flood of memories liberated by the occasion, this plenteous kindness, this bounty of friendship, this reward so much in excess of my merits, well-nigh unmans me.

And, let me add, the noble fullness of style and expression, which I owe to

yourself, and in which the goodwill of the Managers takes corporate form, is in perfect harmony with the spirit which it enshrines.

Of the Managers existent when I joined the Institution, one only remains upon the present Board. The beneficent work of many of them is for ever ended; but I do not forget the sympathy and support which they extended to me during their lives. And now the long line of kindnesses culminates in words and deeds so considerate and appreciative—so representative of their origin in true gentlemanhood, and warmth of heart, that they have almost succeeded in converting into happiness the sadness of my farewell.

With heartfelt prayers for the long-continued honour and prosperity of the Institution which I have served so long, and loved so well,

Believe me, dear Sir Frederick,
Most faithfully yours,
JOHN TYNDALL.

The Managers further reported, That it was Resolved, "That having regard to the distinguished services rendered to the Royal Institution by Dr. Tyndall, he be recommended to the Members for election as Honorary Professor of Natural Philosophy."

It was then moved, and

Resolved, unanimously, "That Dr. Tyndall be nominated for election at the next General Monthly Meeting on Monday, May 9th, as Honorary Professor of Natural Philosophy."

Arthur Gamgee, M.D. F.R.S.
Edward Bagnall Poulton, Esq. M.A. F.G.S. F.Z.S.
Miss Frances Harriet Whitehead,

were elected Members of the Royal Institution.

The Right Hon. Lord Rayleigh, M.A. D.C.L. F.R.S. *M.R.I.* was nominated for election as Professor of Natural Philosophy at the next General Monthly Meeting on May 9.

The following Arrangements for the Lectures after Easter were announced:—

JOHN HOPKINSON, Esq. M.A. D.Sc. F.R.S. B.S. M. Inst. C.E. *M.R.I.*—Four Lectures on ELECTRICITY; on Tuesdays, April 19, 26, May 3, 10.

VICTOR HORSLEY, Esq. F.R.S. B.S. F.R.C.S.—Three Lectures on THE MODERN PHYSIOLOGY OF THE BRAIN AND ITS RELATION TO THE MIND; on Tuesdays, May 17, 24, 31.

THE REV. J. P. MAHAFFY, D.D. Professor of Ancient History in the University of Dublin.—Three Lectures on THE HELLENISM OF ALEXANDER'S EMPIRE: Lecture I. on Tuesday, June 7, MACEDONIA AND GREECE; Lecture II. on Thursday, June 9, EGYPT; Lecture III. on Saturday, June 11, SYRIA.

PROFESSOR DEWAR, M.A. F.R.S. *M.R.I.* Fullerian Professor of Chemistry, R.I.—Seven Lectures on THE CHEMISTRY OF THE ORGANIC WORLD; on Thursdays, April 21, 28, May 5, 12, 19, 26, June 2.

R. VON LENDENFELD, Esq. Ph.D.—Three Lectures on RECENT SCIENTIFIC RESEARCHES IN AUSTRALASIA; on Saturdays, April, 23, 30, May 7.

JOHN W. HALES, Esq. M.A.—Four Lectures on VICTORIAN LITERATURE; on Saturdays, May 14, 21, 28, June 4.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Catalogue of Siwalik Vertebrata. By R. Lydekker. 2 parts. 8vo. 1885-6.
Catalogue of Pleistocene and Prehistoric Vertebrata. By R. Lydekker. 8vo. 1886.
Geological Survey of India. Records, Vol. XX. Part 1. 8vo. 1887.
Memoirs: Palæontologia Indica. Ser. XII. Vol. IV. Part 2; Ser. XIII. Part 1, Fasc. 6. 4to. 1886.
The New Zealand Government—Statistics of the Colony of New Zealand, 1885. fol. 1886.
The Secretary of State for India—Report on Public Instruction in Bengal, 1885-6. fol. 1886.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta; Rendiconti. Vol. III. 1° Semestre, Fasc. 3. 8vo. 1887.
American Philosophical Society—Proceedings, No. 124. 8vo. 1886.
Astronomical Society, Royal—Monthly Notices, Vol. XLVII. No. 4. 8vo. 1887.
Ateneo Veneto—Revista Mensile, Oct. 1885 to Oct. 1886. 8vo.
Bankers, Institute of—Journal, Vol. VIII. Part 3. 8vo. 1887.
British Architects, Royal Institute of—Proceedings, 1886-7, Nos. 11, 12. 4to.
British Museum, The—Catalogue of Ancient MSS. Part 2, Latin. fol. 1884.
Introduction to a Catalogue of the Early Italian Prints. By Richard Fisher. 8vo. 1886.
Descriptive and Historical Catalogue of Japanese and Chinese Paintings. By W. Anderson. 8vo. 1886.
Catalogue of Bengali Printed Books. By J. F. Blumhardt. 4to. 1886.
Subject Index of Modern Works added to the Library in 1880-5. By G. K. Fortescue. 8vo. 1886.
Catalogue of Books in the Galleries in the Reading Room. 8vo. 1886.
Medallic Illustrations of the History of Great Britain and Ireland. By E. Hawkins. Edited by A. W. Franks and H. A. Grueber. 2 vol. 8vo. 1885.
Catalogue of Indian Coins. 1. Muhammadan States. 2. Sultans of Dehli. 3. Greek and Scythic Kings. 8vo. 1885-6.
Catalogue of Greek Coins. Crete and Ægean Islands. 8vo. 1886.
Cambridge Philosophical Society—Proceedings, Vol. VI. Part 1. 8vo. 1887.
Transactions, Vol. XIV. Part 2. 4to. 1887.
Chemical Society—Journal for March 1887. 8vo.
Dawson, George M. Esq. D.Sc. (the Author)—Certain Borings in Manitoba. (Trans. Roy. Soc. of Canada.) 4to. 1887.
Dax: Société de Borda—Bulletins, 2e Serie, Douzième Année, 1e Trimestre. 8vo. 1887.
East India Association—Journal, Vol. XIX. No. 2. 8vo. 1887.
Editors—American Journal of Science for March 1887. 8vo.
Analyst for March 1887. 8vo.
Athenæum for March 1887. 4to.
Chemical News for March 1887. 4to.
Chemist and Druggist for March 1887. 8vo.
Engineer for March 1887. fol.
Engineering for March 1887. fol.
Horological Journal for March 1887. 8vo.
Industries for March 1887. fol.
Iron for March 1887. 4to.
Murray's Magazine for March 1887. 8vo.
Nature for March 1887. 4to.
Revue Scientifique for March 1887. 4to.
Scientific News for March 1887. 4to.
Telegraphic Journal for March 1887. 8vo.
Zoophilist for March 1887. 4to.

Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 27, 28, 29. 8vo. 1887.
Catalogo dei Codici Palatini. Vol. I. Fasc. 1–5. 8vo. 1885–7.
Catalogo dei Codici Panciatichiani. Vol. I. Fasc. 1. 8vo. 1887.
Giornali Politici. 8vo. 1885–6.
Franklin Institute—Journal, No. 735. 8vo. 1887.
Freshfield, Edwin, Esq. LL.D. V.P.S.A. M.R.I. (*the Author*)—Discourse on Some Unpublished Records of the City of London. [Delivered at the Royal Institution.] 4to. 1887.
Geological Institute, Imperial, Vienna—Abhandlungen, Band XII. Nos. 1, 2, 3. fol. 1886.
Jahrbuch, Band XXXVI. Heft 2, 3. 8vo. 1886.
Verhandlungen, Nos. 6–12. 8vo. 1886.
Georgofili Reale Accademia—Atti, Quarta Serie. Vol. IX. Disp. 4. 8vo. 1886.
Hadden, Rev. R. H.—Account of the Old Registers of St. Botolph, Bishopsgate. By Rev. A. W. C. Hallen. 12mo. 1886.
Harlem, Société Hollandaise des Sciences—Archives Neerlandaises, Tome XXI. Liv. 2, 3. 8vo. 1886–7.
Iron and Steel Institute—Journal, 1886, No. 2. 8vo.
Japan, Imperial University of—Memoirs of the Literature College, No. 1. 8vo. 1887.
Johns Hopkins University—American Journal of Philology, No. 28. 8vo. 1886.
Liverpool Literary and Scientific Society—Proceedings, Vols. XXXIX. XL. 8vo. 1884–6.
Manchester Geological Society—Transactions, Vol. XIX. Part 5. 8vo. 1887.
Meteorological Office—Quarterly Weather Report, 1878, Part 2. 4to. 1887.
Monthly Weather Report for September 1886. 4to. 1887.
Weekly Weather Report, Vol. III. No. 53; Vol. IV. Nos. 1–6. 4to. 1886–7.
Report of Meteorological Council, R.S. to 31st March, 1886. 8vo. 1887.
Report of the International Meteorological Committee. 3rd Meeting (1885). 8vo. 1887.
Meteorological Society, Royal—Quarterly Journal, No. 61. 8vo. 1887.
Meteorological Record, No. 23. 8vo. 1887.
Middlesex Hospital—Reports for 1885. 8vo. 1887.
Odontological Society of Great Britain—Transactions, Vol. XIX. Nos. 4, 5. New Series. 8vo. 1887.
Pharmaceutical Society of Great Britain—Journal, March 1887. 8vo.
Photographic Society—Journal, New Series, Vol. XI. Nos. 5, 6. 8vo. 1887.
Preussische Akademie der Wissenschaften—Sitzungsberichte XL.–LIII. 8vo. 1886.
Royal Society of London—Proceedings, Nos. 251, 252. 8vo. 1887.
Sandeman, David, Esq. (*the Author*)—Progress of Technical Education, with special reference to the Glasgow Weaving College. 8vo. 1886.
Saxon Society of Sciences, Royal—Mathematisch-physische Classe: Berichte, 1886. Sup. 8vo.
Society of Arts—Journal, March 1887. 8vo.
Telegraph Engineers, Society of—Journal, No. 64. 8vo. 1887.
United States Geological Survey—Bulletins, Nos. 30–33. 8vo. 1886.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1887: Heft 2. 4to.
Wild, Dr. H. (*Director*)—Repertorium für Meteorologie, Sup. 2, 3, 4. 4to. 1887.
Yorkshire Archæological and Topographical Association—Journal, Part XXXVII. 8vo. 1887.

	Area in Sq. Miles.	Population.
United Kingdom (England, Wales, Scotland, Ireland),	121,500	36,000,000
German Ocean (Heligoland),	0¼	2,000
Mediterranean (Gibraltar, Malta, Cyprus),	4,118	364,000
West Africa (Gambia, Sierra Leone, Gold Coast, Lagos),	15,561	670,000
Atlantic Ocean (Ascension, St. Helena, Falkland Isles),	5,081	7,300
South Africa (Cape, Natal, Basutoland),	273,000	1,748,000
Indian Ocean (Aden, Perim, Mauritius, Borneo, Straits Settlements),	27,810	1,021,150
India, Ceylon, Burmah, China (Hong Kong),	1,477,407	260,160,000
Australasia (Australia, Tasmania, New Zealand, New Guinea),	3,272,000	3,309,000
South Pacific Ocean (Fiji, Rotumah),	8,014	7,000
North America (Canada, Newfoundland),	3,540,000	5,160,000
West Indies, Bermudas, Trinidad, Honduras, Guiana, etc.,	106,035	1,486,000
	8,850,526	309,934,450

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COLONIES, AND

Empire in the year of

JUBILEE

SUMMARY.

	Sq. Miles.	Population.
"The United Kingdom,"	121,500	36,000,000
"The Colonies and India,"	8,729,026	273,934,450
Total of the British Empire,	8,850,526	309,934,450

With a Mutual Trade of £290,453,161.

This Map is prepared and issued in connection with the Appeal for support to the Imperial Institute of the United Kingdom, the Colonies, and India, being

THE NATIONAL MEMORIAL

OF THE

QUEEN'S JUBILEE.

WEEKLY EVENING MEETING,

Friday, April 22, 1887.

H.R.H. The Prince of Wales, K.G. F.R.S.
Vice-Patron, in the Chair.

Sir Frederick Abel, C.B. D.C.L. F.R.S. *M.R.I.*

The Work of the Imperial Institute.

The Colonial and Indian Exhibition, which owes not only its conception, but also its brilliantly successful realisation to your Royal Highness, will be pre-eminently remarkable in times to come, for having achieved many results of vital importance and highest benefit to Her Majesty's subjects in all parts of Her vast Realms.

The collection of all that is commercially valuable and scientifically interesting of the natural products of the great Indian Empire and of the Colonies in one exhibition, embracing as it also did very comprehensive illustrations of the development of commerce, of the arts and of certain industries, in the many countries beyond the seas which combine with the United Kingdom to constitute an Empire over nine million square miles in extent, afforded those at home an opportunity, surpassing all previous conception, of studying and comparing the natural history and resources of those distant lands, of which, attached though we might be individually to one or more of them by ties of friendship or of interest, the knowledge of many of us was of a very vague or partial character.

To the Colonists who visited us last year, the Exhibition has been of inestimable value, in affording them a most favourable and appropriate opportunity of becoming acquainted or renewing their old friendship with the Mother Country, and of examining the progress there made in industrial, educational, and commercial development; in leading to the cultivation of intimacy between Colonists from different sections of the Queen's Dominions; and in affording them invaluable opportunities of comparing the resources and state of development of their respective countries with those of other parts of Europe. No more convincing illustrations than were provided by this great Exhibition could have been conceived of the importance, to the Home Country, to each Colony, and to India, of fostering intimate relationship and unity of action. No more encouraging proof could have been afforded of the desire of all classes of Her Majesty's subjects at home to cultivate a knowledge of those far-off countries which the enterprise and perseverance of the British, and men of British offspring, have converted into prosperous and important Dominions, chiefly during the period of the Queen's reign, than was furnished by the interest which the thousands upon thousands, who

came from all parts, displayed in the study of the instructive collections in the galleries at South Kensington.

It was the success of the Exhibition which led to the definite formulation of the suggestion first made by your Royal Highness in a letter addressed by you in the autumn of 1884 to the Agents-General of the Colonial Governments, that a permanent representation of the resources of the Colonies and India, and of their continually progressing development, might with great benefit to the Empire at large, be established in this country. That the realisation of this idea upon a sufficiently comprehensive basis might constitute a worthy memorial of the accomplishment of fifty years of a wise and prosperous reign; a memorial not personal in its character excepting so far as it constituted an emblem of the love and loyalty of Her Majesty's subjects, but tending, as she would most desire, to serve the interests of the entire Empire, had only to be pointed out by your Royal Highness to be heartily concurred in by the Official Representatives of the Colonies and India, who were so intimately identified with the triumphs of the recent Exhibition.

The Committee to whom you, Sir, entrusted the elaboration of a scheme for carrying this conception into effect, became persuaded by a careful consideration of the subject that such an Institution as your Royal Highness desired to see spring into life, to be a memorial really worthy of the Jubilee of Her Majesty's reign and to fulfil the great purposes which you had in view, must not be confined in its objects to particular portions of the Queen's Dominions, but must be made thoroughly representative of the interests and of the unity of the whole Empire.

The outline of the scheme for the establishment of an Imperial Institute for the United Kingdom, the Colonies and India, which met with the cordial approval of your Royal Highness, was necessarily concise in dealing with the very wide extent of ground which the operations of the Institute are intended to cover; but those who have carefully considered it and rightly interpreted its proposals, have not failed to realise that it aims at very much more than the creation and maintenance of collections, illustrative of the natural resources of our Colonies and of India, and of the development and present condition of the chief industries of different parts of the Empire.

One of the primary objects of the Institute will certainly be the establishment of thoroughly well selected, carefully arranged, and efficiently maintained representations of the natural products which constitute the treasures, and are emblematic of the important positions in the Empire, of those great Colonial possessions which, during the fifty years of Her Majesty's reign, have, in many instances, experienced a marvellous development in extent, in commercial, social, and even in political importance.* The recent

* Statistical statements illustrating the development of the Colonies during the Queen's reign are appended.

Exhibition not only afforded conclusive demonstration of the great interest and value to the United Kingdom which must attach to such collections if properly organized; by such illustrations as the magnificent collections of valuable woods, from nearly every Colony, many quite unknown in England, and the great variety of valuable economic products from India, of the existence of which we at home had little idea, it also served to convince us that our knowledge of the great countries which constitute the chief portion of the Empire is very limited and imperfect, and that their resources are in many directions still in the infancy of development. Our Colonial Brethren cannot, on their part, fail to be greatly benefited by being thoroughly represented in a well-selected and carefully organised assemblage of illustrations of the sources of prosperity which constitute the sinews of their commerce, and upon a continued exploration and cultivation of which must depend the maintenance of their influence upon industrial and social progress. Neither can they fail to reap substantial advantages by pursuing a friendly rivalry with each other in demonstrating the advances made from time to time in the development of the resources of the respective portions of the Empire in which their lot is cast.

The hearty co-operation and important material support to which the great Colonies, through their representatives in London, pledged themselves when the scheme for the proposed Imperial Institute was in the first instance limited to this branch of the great work which it is now contemplated to accomplish, afforded conclusive evidence of their earnest desire to be in all respects thoroughly represented in the Mother Country, and to take their places permanently in our midst as fellow-labourers in the advancement of the prosperity of the Empire. In furtherance of this important end, a notable feature of that building which, in its character, will, it is hoped, be worthy of the momentous epoch it is destined to commemorate, will be, the attractions and conveniences presented by it as a place of resort and a *rendezvous* for Colonists visiting England, and, it is also anticipated, for the important Societies which represent the Colonies and Asiatic possessions in this Country, and the facilities which it will afford for reference to literature concerning the Colonies and India, for conferences on matters of common interest and value to the Colonists and those at home, for the interchange of information between the British manufacturer and those in the Colonies who are directly interested in meeting his requirements, and generally, for the cultivation of intimate relations and good fellowship between ourselves and our brethren from all parts of the Empire.

The Institute will, however, not only operate actively under its own roof in promoting the cultivation of a better knowledge of the geography, natural history, and resources of our Colonies, and for the advancement of the interests of the Colonists in this Country; it is also contemplated that representative collections of the natural products of the Colonies and India, carefully identified with the

more elaborate collections of the head establishment, shall be distributed to provincial centres, and that the provinces shall be kept thoroughly conversant with the current information from the Colonies and India, bearing upon the interests of the commercial man, the manufacturer, and the intending emigrant.

Although the formation, and maintenance up to date, of collections illustrative of the development and present condition of the important Industries of the Empire also forms, as I have stated, a part of the programme of the Institute, the scope of its activity in relation to industry will be of a much more comprehensive character; indeed, it is to be hoped that the work which it will achieve in furtherance of the development and progress of industries and their future maintainance in the United Kingdom at least upon a footing of equality with their conditions in the great Continental States, will be most prominent in securing to the Imperial Institute the exalted position which it should occupy as the National Jubilee Memorial of Her Majesty's reign.

There is no need for me to recall to the minds of an audience in the Royal Institution the great strides which have been made during the last fifty years in the applications of science to the purposes of daily life, to the advancement of commerce, and to the development of the arts and manufactures. Nor is it necessary to dwell upon the fact that this country is the birth-place of the majority of the great scientific and practical achievements which have revolutionised means of intercommunication, and have in other ways transformed the conditions under which manufactures, the arts and commerce are pursued. These very achievements, of which we as a Nation are so justly proud, have led, however, by many of their results, to our becoming reduced to an equality of position with other prominent Nations in regard to important advantages we so long derived from the possession in this country of great material resources easy of access and application, and from the consequent pre-eminence in certain branches of trade and industry which we so long enjoyed.

In 1852, Sir Lyon Playfair, in one of a course of most interesting lectures on some of the results of the preceding year's great Exhibition, was impelled by the teaching of that great world's display, to point out that "the raw material, formerly our capital advantage, was gradually being equalised in price and made available to all by the improvements in locomotion," and "that industry must in future be supported not by a competition of local advantages, but by a competition of intellect." If this was already felt to be the state of the case six-and-thirty years ago, how much more must we be convinced of the full truth of this at the present day, by the conditions under which the British merchant and manufacturer have to compete with their rivals on the Continent and in the United States.

It is still within the recollection of many that almost the whole world was in very great measure dependent upon Great Britain for its supplies of ordinary cast iron. Even as lately as 1871, the

United States of America received from Great Britain nearly one-fifth of its total produce of pig iron; but from 1875 all importation of British iron ceased for over three years, and it was only in consequence of requirements in the States exceeding the capabilities of production, that some small demands arose in 1879, which were for some time maintained.

But while, in 1879, the pig iron produced in the United States amounted to little over 3,000,000 tons, in 1882 the make had increased by 70 per cent., viz. to over 5,100,000 tons. Since that time the actual make has not increased (in 1885 it amounted to 4,529,869 tons of 2000 lb.), but the *capacity of production*, which vitally interests the iron trade of this country, has risen enormously, the present capacity of all the American pig-iron works being estimated at over 8,900,000 tons, or nearly 300 per cent. greater than it was in 1879. So much regarding the United States; looking nearer home, we find that the iron of France, Belgium, and Germany not only competes with ours in the open market, but that Belgian and German iron is actually imported into this country to a moderate extent.

As an instructive illustration of the advance and influence of the improvements which have been made in intercommunication upon the value of our natural products and their importance even in our own industries, I may, on the authority of Sir Lowthian Bell, state the astounding fact that in the opinion of competent authorities, the ore (hæmatite) especially suitable for steel manufacture by the Bessemer process can be brought over sea a distance of 1000 miles, landed close to mines furnishing the cheapest made pig iron of Great Britain, and converted into *steel* rails at a lower cost than the native ironstone of Cleveland can furnish similar rails in *iron*.

From time to time the ground which we have lost through the development of the resources of other countries has been more than retrieved temporarily by improvements effected through the more thorough comprehension and consequent better application of the scientific principles underlying processes of manufacture. Thus the quantity of fuel consumed in producing wrought-iron rails has been gradually reduced by improvements in the construction and working of furnaces, until less than one-half the amount is now required per ton of such rails than was employed fifty years ago; but, remarkable as it may seem, the ultimate effect of an advance of this importance is actually to improve the position, in relation to this manufacture, of other Nations less favourably circumstanced than Great Britain in the matter of coal, for, instead of having to multiply any difference in our favour in the cost of fuel required to produce a ton of rails by twelve, that difference has now only to be multiplied by three in order to arrive at the extent of our advantage.

The history of the development of steel manufacture during the last twenty-five years affords a most instructive illustration of the fluctuations which may ensue in the value of our natural resources, and the consequent condition of one or other of our important industries,

arising out of continued advances made in the application of science to the perfection or transformation of manufacturing processes, and of the stimulating effects of such fluctuations upon the exertions of those who are able to bring scientific knowledge to bear upon the solution of problems in industrial operations which entirely baffle the ordinary manufacturer. Within that period the inventions of Bessemer and of Siemens have led to the replacement of iron by steel in some of its most extensive applications. The Bessemer converter, by which pig iron is rapidly transformed into steel by the injection of air into the molten metal, has, so far as this country is concerned, to a very great extent superseded the puddling furnace, in which pig iron is transformed by long-continued laborious treatment into steel or malleable iron. This important change in our national industry was, ere long, productive of a serious crisis therein, and for the reason that the pig iron produced from a large proportion of those ores which, from their abundance and the cheapness of their treatment, have been largely instrumental in placing Great Britain in her high position as an iron-producing nation, could not be applied to the production of marketable steel by means of the Bessemer converter. In the purification of this pig iron during its conversion in the puddling furnace into a suitable material for the production of rails, the elementary constituent phosphorus, which it had carried with it from the ore as a contaminating ingredient very detrimental to its strength, was eliminated, and by sufficient treatment a malleable iron of good quality was obtained; but in the production of steel from the same material in the Bessemer converter, the phosphorus is almost entirely retained in the metal, rendering it unsuitable for manufacture into rails or plates. Hence the application of this rapid steel-making process had to be chiefly restricted to particular kinds of ores, the supplies of which are limited to a few districts in this country. These had to be largely supplemented by importations from other countries; nevertheless the cheapness of production and superiority in point of strength, durability and lightness of the steel rails thus sent into the market from the Bessemer converter combined to maintain a supremacy of them over iron rails, &c., manufactured by the old puddling processes from the staple ores of the country.

The advantages presented by steel over the wrought iron of the puddling furnace for constructive purposes speedily became evident; combining as it does nearly double the strength with a more than proportionate superiority in elasticity and ductility, its value for ship-building purposes did not long fail to be realised. It was soon found more profitable to build a steel steamer, paying a price of nearly 9*l.* per ton for the material, than to construct one of iron which cost only 6*l.* 5*s.* per ton. The effect of the rapid displacement of malleable iron by steel produced from ores of a particular class has been, that at least 85 to 90 per cent. of the iron ores of Great Britain could no longer be applied to the production of material for rails and for con-

structive purposes, being unavailable for steel-making by any method which could compete with the Bessemer and Siemens processes. Great has been the apprehension among the owners of those ores, that the demand for iron which they can furnish could not revive, but the scientific metallurgist has successfully grappled, from more than one direction, with the great problem of restoring their commercial importance.

Modifications of the mode of working the rival of the Bessemer process, namely, the open-hearth (Siemens-Martin) process, have given successful results in the production of serviceable rails containing higher proportions of phosphorus than had before been admissible, and a simple alteration of the method of carrying out the Bessemer process has, within the last few years, led to really triumphant results, with the employment of those ores which, before, could only be dealt with by the searching operation of the old puddling furnace. By utilising the basic character of lime during the treatment of the melted pig iron, yielded by phosphoric ores, with air in the Bessemer converter, the phosphorus is fixed at the moment of its elimination by oxidation from the metal, and the objectionable impurity is held bound in the slag, while a steel is obtained rivalling in freedom from phosphorus the product furnished by the pure varieties of English and foreign ore which alone could previously be successfully dealt with by the Bessemer process. This modified treatment of iron for the production of steel, called the *basic treatment*, was soon applied also to the open-hearth (the Siemens and Siemens-Martin) processes of steel-making; thus a new era was established in steel manufacture by the quick processes, there being now but very few restrictions to their application to iron produced from all varieties of ores. Indeed, the treatment is actually being applied profitably to the recovery of iron from the rich slag forming the refuse-product of the puddling furnace in the production of malleable iron, which, containing as it did the phosphorus eliminated from the pig iron by the laborious purifying treatment, had been condemned to limited usefulness as a material for road-making, while now it ranks in market value with some ores of iron. Yet another most interesting and valuable result has been achieved by this simple application of scientific knowledge. The slag or refuse-product of the basic treatment of iron contains, in the form of phosphates of lime and magnesia, the whole of the phosphorus which it is the main function of that treatment to separate from the metal; it was soon found that the phosphoric acid which had been produced by the elimination of the pernicious element in the conversion of bad iron into good steel, existed in this refuse slag in a condition as readily susceptible of assimilation by plants as it is in the valuable artificial manure known as superphosphate; the slag, simply ground up, constitutes therefore a manure which is already of recognised value and commands a ready sale at very profitable prices.

The organisation of this latest advance in the development of

steel manufacture dates back only nine years, and already the year's product of the basic process amounts to over 1,300,000 tons of steel. But although it is to Englishmen that the owner of iron property and the steel-maker are again indebted for these important results, and to English manufacturers that the first practical demonstration of the success of this process is due, its application has been far more rapidly elaborated upon the Continent than here; in Germany the importance of the subject was at once realised, and it is there that considerably the largest proportion of steel is produced by the basic treatment; it is in Germany also that the value of the slag for agricultural purposes has been developed; the first steps in its utilisation here being but just now taken, in Staffordshire.

I have already referred to the remarkable strides which have been made in the extension of iron manufacture in the United States: the development there of steel production has been no less marvellous. In 1879, 928,000 tons of Bessemer steel were produced; in 1885 the make amounted to 1,701,000 tons, while the productive capacity in that year was estimated at 4,102,000 tons. With other extensive steel-producing works in course of completion, provision is being made for increasing the power of production by another million tons. Looking to the fact that at the present time the railway mileage in the United States exceeds that of the whole of Europe, there being 1,300,000 miles of railway in operation, while at the beginning of 1865 there were only 34,000 miles, the causes of this enormous development of the iron and steel manufacture are evident; the resources of the country in ore and fuel are gigantic, and the systematic technical training of the people has made its influence felt upon the development of this as of every other branch of industry which our friendly rivals pursue. But it is not only in the United States that the development in the production of iron and steel has greatly increased of late years; thus, in Germany the increase in the production of pig iron alone, during the last twenty-one years, has been 237 per cent., in Austro-Hungary 152 per cent., while the increase in France and Belgium is 64 per cent., and therefore not greatly inferior to our own (75 per cent.).

Although, however, the increase in actual production of iron and steel in this country has not kept pace with that of some other countries, it is satisfactory to know that our *productive power* has very greatly increased in late years, and there is probably no one branch of our industries in which we have maintained our position so satisfactorily in regard to quality of product as that of iron and steel manufacture, even although, every now and then, we have indications that in the struggle with other Nations for superiority of product and for pre-eminence in continuity of progress, we have to look to our laurels. While this Country owes a deep debt of gratitude to such men as Neilson, Mushet, Bessemer, Siemens, Thomas and Gilchrist, who by their brilliant discoveries and inventions have maintained Great Britain's posi-

tion as the leader in the origination of successive eras of advance in iron and steel manufacture, there is no question that the trade generally has in recent years derived the greatest assistance and benefit from the organisation of the Society which, under the name of the Iron and Steel Institute, has brought the members of the trade to recognise that they themselves, and the country, reap incalculable benefit from their free interchange of knowledge and the results of experience, their candid discussion of successes, failures, and diversities of views and practice—the combination of friendly rivalry with hearty co-operation in the advancement of the science and practice of their important calling.

While we have succeeded in maintaining a foremost position in iron and steel manufacture, there are some other important branches of industry, for a time essentially our own, the present condition of which, in this country, we cannot contemplate with equal satisfaction. Several instructive illustrations might be quoted, but I will content myself with a brief examination of one of the most interesting.

A glance at the history of the utilisation of some products of the distillation of coal will present to us an industry created and first elaborated in England, which has, on the one hand, by its development effected momentous changes in other industries and in important branches of commerce, while on the other hand it has been in great measure wrested from us in consequence of the systematic collaboration of scientific and practical workers on the Continent.

In discussing the recent advances made in chemical manufactures as exemplified by the Exhibition of 1851, Playfair, in the lecture to which reference has already been made, spoke of the great development of the value of the evil-smelling tar, which was then made to furnish the solvent liquids benzene and naphtha, and the antiseptic creosote, the residual material being utilised for pavements and for artificial fuel. The chemist little dreamt then that between 1851 and the year of the next great Exhibition, 1862, coal tar would have become a mine of wealth equally to science, to manufactures and to the arts, in which fresh workings have ever since continued to be opened up, and still present themselves for exploration. Hofmann, in his valuable report on the chemical products and processes elucidated by that Exhibition, dwells with the enthusiasm of the ardent worker in science upon the brilliant products obtained from coal tar, which had resulted from the labours of the scientific chemist and had already acquired an almost national importance, although this great industry was then still in its infancy. From the year 1856, when the first colouring matter known as *Mauve*, was discovered and manufactured by a young student at the College of Chemistry, Mr. Perkin, one of Hofmann's most promising pupils, to the present time, the production of new coal-tar colours or of new processes for preparing the known colours in greater purity, has progressed uninterruptedly, this industry having long since become one of the most important, and

also one of the most remarkable, as illustrating by each stage of its development the direct application of scientific research to the attainment of momentous practical results.

It is interesting to note that Perkin's discovery of mauve, as a product of one of the most important derivatives of coal-tar, called aniline, was arrived at in the course of an investigation, having for its object the artificial production of the invaluable vegetable alkaloid, quinine, the synthesis of which has been the aim of many researches during the past half century, and appears to be at length about to be achieved, as the result of a long chain of scientific research. The difficulties to be overcome before mauve could be produced upon a manufacturing scale were very great, and were only solved by a steady pursuit of scientific research, side by side with practical experiments suggested by its results. Aniline—the parent of the first coal-tar colour, a liquid organic alkali—a most fertile source of interesting and important discoveries in organic chemistry, which have made the names of Hofmann and others famous—was produced with difficulty by various methods in very small quantities, so as to be almost a chemical curiosity at the time of the discovery of mauve. Among the substances from which it had been prepared was the volatile liquid known as *benzene*, first discovered in the laboratory of this Institution in 1825 by Faraday, in the liquid products condensed from oil gas, but afterwards obtained by Mansfield, in the College of Chemistry, from coal-tar naphtha, which also furnished in his hands a series of homologous liquids, many of them now of great importance as the raw materials from which dyes are obtained.

The conversion of benzene into aniline, which had been effected on a very small scale in different ways by German and Russian investigators, was accomplished as a manufacturing process after many difficulties by Perkin, and within a year after the discovery of mauve by him, it was in the hands of the silk dyer. Perkin's success led other chemists at once to pursue researches in the same direction, especially in France, where the next important coal-tar colour, *magenta* or fuchsine, was obtained, by M. Verquin, the successful manufacture of which in a pure state was, however, first accomplished by English chemists, with Mr. E. C. Nicholson at their head, whose magnificent specimens in the 1862 Exhibition excited universal admiration. In 1861 beautiful violet and blue colours were produced, again by French chemists (Girard and De Laire), but were manufactured shortly afterwards in a pure state by Nicholson. This brought the coal-tar dye industry down to the year 1862, and Hofmann, in congratulating his young pupil Perkin (in his Jury Report) upon the splendid industrial result achieved, in having first manufactured a colour from coal-tar, which had been arrived at by purely scientific research, expressed the hope that the commercial success of his enterprise might not divert him from the path of scientific inquiry—a hope which he has lived to see fully realised, as the long series of fresh contributions, made almost without inter-

ruption since that time by Perkin to our knowledge of organic chemistry have been among the most brilliant and important achieved by chemists of the present day, and have continued to influence in a most important manner the branch of industry which he created.

The six years succeeding those which formed the first period (1854–1862) of existence of this industry were fruitful, not only of new colours but also of progress made in England, as well as on the Continent, in the development of the manufacture, and of our knowledge of the constitution, of the beautiful dyes which outvie each other in brilliancy. Important researches by Hofmann, which, while establishing the correctness of his scientific conceptions of the real nature of magenta, led to the discovery, by him, of a matchless violet dye, were followed by the production, at the hands of Perkin and Nicholson in England, and of several workers on the Continent, of the well-known gas-light greens, of Bismarck brown, and of some eight or nine other important dyes; blue, yellow, orange, and scarlet.

In the next period of six years (1868–1874) another great stride was made in the coal-tar colour industry, due to important scientific researches carried out by two German chemists, Graebe and Liebermann, which led them, in the first place, to obtain an insight into the true nature of the colouring matter of one of the most important staple dye-stuffs, namely the Madder root. They found that this colouring matter which chemists call *Alizarine* was related to *Anthracene*, one of the most important solid hydrocarbons formed in the distillation of coal, a discovery which was speedily followed by the artificial formation of the madder dye, alizarine, from that constituent of coal-tar. At first, this achievement of Graebe and Liebermann was simply of high scientific interest, but Perkin, who was pursuing research in the same direction, soon discovered two methods by which the conversion of anthracene into the madder dye could be accomplished on a large scale, and one of these, which was also arrived at by the German chemists simultaneously with Perkin, is still used for the manufacture of alizarine, which was for some time most actively pursued in this country, with very momentous results, as regards the market value of the madder root. The latter has long been most extensively cultivated in Holland, South Germany, France, Italy, Turkey, and India, the consumption of madder in Great Britain having attained to an annual value of as much as 1,000,000*l.* sterling. Playfair pointed out in 1852 that important improvements had been attained in the extraction of the red colour or alizarine from the madder root, the refuse of which, after removal of the dye in the ordinary way, had been made, by a simple treatment, to furnish further quantities of the colouring matter. This result, most valuable at the time of the first great Exhibition, became insignificant when once the dye was artificially manufactured from anthracene; the price paid for madder in 1869 was from 5*d.* to 8*d.* per pound, but now the equivalent in artificial madder dye, or alizarine, of one pound of the root, can be obtained for one-halfpenny. The latter is still used by the most conservative

section of the dye-trade, the wool-dyers (and in some respects it appears to present in this direction a little advantage over the artificial colour), but the value of its present annual consumption in Great Britain has become reduced from one million to about 40,000*l.* During the development of the artificial alizarine industry within this third period of six years, the continued researches of Perkin, Schunck, Baeyer, Caro, and others have led to the development of further important varieties of coal-tar dyes, the most valuable of which, discovered by the two last-named chemists, was a beautiful *cerise* colour, called eosine.

With the discovery of artificial alizarine the truly scientific era of the coal-tar industry may be said to have commenced, most of the commercially valuable dye-products, obtained since that time, being the result of truly theoretical research by the logical pursuit of definite well understood reactions. The wealth of discovery in this direction made during the last thirteen years is a most tempting subject to pursue, but I am compelled to refrain from entering upon it, further than to point out that the practical significance of beautiful scientific researches of many years previous became developed—that one of the results was the production of very permanent and brilliant scarlet and red dyes, the manufacture of which has greatly reduced the market value of cochineal—that the careful study of the original coal-tar colours led to their production in a state of great purity by new and beautifully simple scientific methods (which include the extensive employment as an invaluable practical agent in their production, of the curious gaseous oxychloride of carbon, until lately a chemical curiosity, produced through the agency of light, and hence christened *phosgene gas*, by its discoverer, John Davy, in 1812); and lastly, that even the well-known vegetable colouring matter, *indigo*, one of the staple products of India, now ranks among the colours synthetically obtained by the systematic pursuit of scientific research, from compounds which trace their origin to coal-tar.

The rapid development of the coal-tar colour industry has not failed to exercise a very important beneficial influence upon other chemical manufactures; thus, the distillation of tar, which was a comparatively very crude process, when, at the period of the first Exhibition, benzene, naphtha, dead-oil and pitch were the only products furnished by it, has become a really scientific operation, involving the employment of comparatively complicated but beautiful distilling apparatus for the separation of the numerous products which serve as raw materials for the many distinct families of dyes. Very strong sulphuric acid became an essential chemical agent to the alizarine manufacturer, and, as a consequence, the so-called anhydrous sulphuric acid, the remarkable crystalline body which was for many years prepared only in small quantities from green-vitriol, and of which minute specimens carefully sealed up in glass tubes were preserved as great curiosities in my student's days, is now

made at a low price upon a very large scale by a beautifully simple process worked out in England, by Squire and Messell. The alkali and kindred chemical trades have been very greatly benefited by the large consumption of caustic soda, of chlorate of potash and other materials used in the dye manufactures, and the application of constructive talent, combined with chemical knowledge, to the production of efficient apparatus for carrying out on a stupendous scale the scientific operations developed in the investigator's laboratory, has greatly contributed to the creation of a distinct profession, that of the chemical engineer.

One of the most beneficial results of the rapid development of the coal-tar colour industry has been its influence upon the ancient art of dyeing, which made but very slow advance until the provision of the host of brilliant, readily applicable colours completely revolutionised both it and the art of calico printing.

In endeavouring to furnish some idea of the magnitude of the coal-tar colour industry, I may state that the total value of the coal-tar colours produced in 1885 amounted to about 3,500,000*l.* The value of the alizarine and its related dyes which are used with it for obtaining various shades of colour, now amounts to about one-half of the total produce of the coal-tar colour industry. Their manufacture in England in considerable quantities still continues, but it is a suggestive fact that the value of the artificial alizarine imported into this country from the Continent last year, was 259,795*l.* Taking the average value of madder at 5*d.* per lb., and the cost of its equivalent in artificial alizarine at one-halfpenny, the quantity imported, if valued at 5*d.* per lb., would represent about 2,597,950*l.*

I venture to think that it will be interesting at this point, to quote some words of prophecy included in Professor Hofmann's important 'Report on the Chemical Section of the Exhibition of 1862,' and to inquire to what extent they have been verified. In commenting upon one of the features of greatest novelty in that world's show, the exhibition of the first dye products derived from coal-tar, he says :—

"If coal be destined sooner or later to supersede, as the primary source of colour, all the costly dyewoods hitherto consumed in the ornamentation of textile fabrics ; if this singular chemical revolution, so far from being at all remote, is at this moment in the very act and process of gradual accomplishment ; are we not on the eve of profound modifications in the commercial relations between the great colour-consuming and colour-producing regions of the globe ? Eventualities, which it would be presumptuous to predict as certain, it may be permissible and prudent to forecast as probable ; and there is fair reason to believe it probable that, before the period of another decennial Exhibition shall arrive, England will have learnt to depend, for the materials of the colours she so largely employs, mainly, if not wholly, on her own fossil stores. Indeed, to the chemical mind it cannot be doubtful, that in the coal beneath her feet lie waiting to be drawn forth, even as the statue lies waiting in the quarry, the fossil equiva-

lents of the long series of costly dye materials for which she has hitherto remained the tributary of foreign climes. Instead of disbursing her annual millions for these substances, England will, beyond question, at no distant day become herself the greatest colour-producing country in the world ; nay, by the strangest of revolutions, she may ere long send her coal-derived blues to indigo-growing India, her tar-distilled crimson to cochineal-producing Mexico, and her fossil substitutes for quercitron and safflower to China, Japan, and the other countries whence these articles are now derived.

"Coal and iron, it has been said, are kings of the earth, and our latest chemical victories seem destined to add another vast province to the dominion of coal, and a fresh element of commercial predominance to its already powerful possessors."

So far as concerns the displacement of madder, cochineal, quercitron, safflower, and other natural dye materials from their positions of command in the markets of England and the world, Hofmann's predictions have been amply fulfilled, and it appeared, in the earlier days of the coal-tar colour industry, as though he would be an equally true prophet in regard to England becoming herself the greatest colour-producing country in the world. But, although Germany did little in the days of infancy of this industry, beyond producing a few of the known colours in a somewhat impure condition, many years did not elapse ere she not only was our equal in regard to the quality of the dyes produced, but, moreover, had outstripped us in the quantities manufactured and in the additions made to the varieties of valuable dyes sent into the market. The following is the estimated total value of coal-tar colours manufactured in the several producing countries as far back as 1878:—Germany, 2,000,000*l.*; England, 480,000*l.*; France, 350,000*l.*; Switzerland, 350,000*l.* These figures show that the value of the make of colours in England was less than one-fourth that of Germany, and that even Switzerland, which, in competing with other countries industrially is at great natural disadvantages, was not far behind us, ranking equal to France as producers. The superior position of Germany in reference to this industry may be in a measure ascribable to some defects in the operation of our Patent Laws and to questions of wages and conditions of labour; but the chief cause is to be found in the thorough realisation, by the German manufacturer, of his dependence for success and continual progress upon the active prosecution of scientific research, in the high training received by the chemists attached to the manufactories, and in the intimate association, in every direction, of systematic scientific investigation with technical work.

The young chemists which the German manufacturer attracts to his works rank much higher than ours in the general scientific training which is essential to the successful cultivation of the habit of theoretical and experimental research, and in the consequent appreciation of, and power of pursuing, original investigations of a high order. Moreover, the research laboratory constitutes an integral

part of the German factory, and the results of the work carried on by and under the eminent professors and teachers at the universities and technical colleges are closely followed and studied in their possible bearings upon the further development of the industry.

The importance attached to high and well-organised technical education in Germany is demonstrated not only by the munificent way in which the scientific branches of the universities and the technical colleges are established and maintained, but also by the continuity which exists between the different grades of education; a continuity, the lack of which in England was recently indicated by Professor Huxley with great force. Nearly every large town in Germany has its "Real Schule," where the children of the public elementary schools have the opportunity, either by means of exhibitions or by payment of small fees, of receiving a higher education, qualifying them in due course to enter commercial or industrial life, or to pass to the universities or to the polytechnic or technical high schools, which, at great cost to the Nation, have been developed to a remarkable extent in recent years, and have unquestionably exercised a most beneficial influence upon the trade and commerce of the country. A most important feature in the development of these schools is the subdivision of the work of instruction among a large number of professors, each one an acknowledged authority in the particular branch of science with which he deals. Thus, at the Carlsruhe Polytechnic School—one of the very earliest of its kind —which was greatly enlarged in 1863—the number of professors is 41; and at Stuttgart the teaching staff of the polytechnic school amounts to 65 persons, of whom 21 are professors.

The important part taken by the German universities in the training of young men for technical pursuits has often been dwelt upon as constituting a striking feature of contrast to our university systems. The twenty-four universities in the German Empire, each with its extensive and well-equipped science departments and ample professional staff, contribute most importantly to the industrial training of the Nation in co-operating with the purely technical schools. The facts specified in the Report of the Technical Education Commission that, in the session 1883–4, there were 400 students working in the chemical laboratories at Berlin, and that, during the same session, 50 students were engaged in original research at Munich (where the traditions of the great school of Liebig are worthily maintained), illustrate the national appreciation of the opportunities presented for scientific training; and the expenditure of 30,000*l.* upon the physical laboratory, and 35,000*l.* upon the chemical department, of the New University of Strasbourg, serves to illustrate the unsparing hand with which the resources of the country are devoted to the provision of those educational facilities which are the very life-spring of the industrial progress whence those resources are derived.

In France, advanced education had been allowed to sink to a low ebb after the provincial universities had been destroyed in the great

Revolution, and the University of Paris had been constituted by the first Napoleon the sole seat of high education in the country. Before the late war, matters educational were in a condition very detrimental to the position of the country among Nations. There was no lack of educational establishments, but the systems and sequence of instruction lacked organisation.

Since the war, France has made great efforts to replace her educational resources upon a proper footing. The provincial colleges have been re-established at a cost of 3,280,000*l.*, and the annual budget for their support reaches half-a-million. The organisation of industrial education has now been greatly developed, though still not on a footing of equality with that of Germany. The practical teaching of science commences already in the elementary schools, and the groundwork of technical instruction is afterwards securely laid by the higher elementary schools, of which so many excellent examples are now to be found in different parts of France. Every large manufacturing centre has its educational establishment where technical instruction is provided, with special reference to local requirements; the Institute Industriel, at Lisle, and the Ecole Centrale of Lyons, are examples of these. In order to render these colleges accessible to the best talent of France, more than 500 scholarships have been founded, at an annual cost of 30,000*l.* The Ecole Centrale des Arts et Manufactures, of Paris, still maintains the reputation as the great technical university of the country, which it earned many years ago, and receives students from the provincial colleges, where they have passed through the essential training preliminary to the high technical education which that great institution provides.

Switzerland has often been quoted as a remarkable illustration of the benefits secured to a Nation by the thoroughly organised education of its people. Far removed from the ocean, girt by mountains, poor in the mineral resources of industry, she yet has taken one of the highest positions among essentially industrial Nations, and has gained victories over countries rich in the possession of the greatest natural advantages. Importing cotton from the United States, she has sent it back in manufactured forms, so as to undersell the products of the American mills. The trade of watch-making, once most important in this metropolis, passed almost entirely to Switzerland years ago; the old established ribbon trade of Coventry has had practically to succumb before the skilled competition of Switzerland, and although she has no coal of her own, Switzerland is at least as successful as France in her appropriation of the coal-tar colour industry and her rivalry in rate of production with England, the place of its birth and development. Comparative cheapness of labour will not go very far to account for these great successes; they undoubtedly spring mainly from the thoroughly organised combination of scientific with practical education of which the entire people enjoys the inestimable benefit.

From the age of six to twelve, or thirteen, the children must attend primary schools, where, as the pupils advance in age, the instruction becomes more practical. The application of the knowledge acquired in these primary schools, is cultivated for three years at the so-called "Improvement Schools," and upon these follow the Cantonal High Schools, which are divided into trade- and classical schools, and of which there are sixty-seven in the little canton of Zurich alone. Above those there are five universities and the Zurich Technical Institute, which is supported by the Federal Government, the Canton itself subscribing liberally to its aid. It owns a very numerous staff of professors and teachers, and the number of students attending is so large that, magnificent as was the accommodation which it already afforded, no less than 50,000*l.* have recently been spent upon additional chemical laboratories. Although the Germans have so many technical colleges and chemical schools they go in large numbers to the Zurich Institute, and even a few English appreciate the great advantages which must accrue from the thorough training attainable in this world-renowned school of technics.

Holland furnishes another brilliant example of the success with which a Nation can bring the power of systematic technical education to bear in securing and maintaining industrial victories in the face of most formidable disadvantages, while the United States of America, so rich in natural resources, have long since realised the immensity of additional advantages to be gained over European Nations in the war of industry by a wide diffusion and thorough organisation of technical education. So long as forty years ago the States already possessed several excellent educational institutions established upon the basis of the Continental polytechnic schools, but it was not until about fifteen years later that the great advances achieved by Germany in technical education, made America, like France, anxious concerning the progress and development of some of her industries.

The subject was at once made a thoroughly national one, and it is now just upon a quarter of a century ago since Congress ordained that each State should provide at least one college, having for its leading objects the diffusion of scientific instruction in its relations to the industry of the country, and decreed that public lands should be granted to the States and Territories providing such colleges. In accordance with the system adopted for the regulation of these grants, the State of New York received close upon a million acres of land, and out of this grant grew the University of Cornell, which could be called upon to educate 500 students free of charge under the conditions of the grant, and which was already at work in 1867, having in the meantime received most important aid from an endowment of 100,000*l.* by a private citizen, Mr. Cornell. The combined effect of this State action and of great private munificence, was a remarkably rapid development of scientific and technical education throughout the country; besides some fifty colleges, with

eight or nine thousand students, which sprang out of the Land Grant Act for Industrial Education, there are now in the States about 400 other universities and colleges (with 35,000 students, and between 5000 and 6000 teachers), in a large proportion of which efficient instruction in applied science is provided.

Among the more prominent of America's technical schools are the Stevens Institute of Technology, New Jersey; the Pennsylvania Polytechnic College, Philadelphia; the Lawrence Science School, in connection with Harvard University; the Columbia College and School of Mines, New York; the Massachusetts Institute of Technology, Boston; the Engineering School of the Michigan University; the Lafayette College, Pennsylvania; the Mechanical College of Louisiana University; the Brown University, Rhode Island; Washington College, Virginia; Union College, Schenectidy; and the Shipley School, in connection with the Cornell University. To the useful work accomplished within a few years by these and many other highly important educational institutions, which have placed the acquisition of scientific knowledge within the reach of the very humblest, the enormous strides made by the United States in the development of home industries must unquestionably be in the main ascribed.

While extolling the comprehensive and well-organized systems of technical education existing in all parts of the Continent and the United States, let us not undervalue the great progress which has been made in recent years in Great Britain in the advancement and extension of technical instruction. The Royal Commission on the Depression of Trade and Industry state, as the result of evidence collected by them, that "It would be difficult to estimate the extent to which our industries have been aided in various ways by the advance of elementary, scientific, and technical education during the last twenty years."

The important influence exercised by the admirable work which the organisation of the Science and Art Department has accomplished, upon the intellectual and material progress of the Nation, is now thoroughly recognised. Professor Huxley, the Dean of the Normal School of Science, in his recent important letter "On the organisation of industrial education," has reminded us that "the classes now established all over the country in connection with that department, not only provide elementary instruction accessible to all, but offer the means whereby the pick of the capable students may obtain in the schools at South Kensington as good a higher education in Science and Art as is to be had in the country," and "that it is from this source that the supply of science and art teachers is derived, who in turn raise the standard of elementary education" provided by the School Boards. The extension of facilities for the education of those engaged in art-industries is constantly aimed at, as was recently demonstrated by the creation of free studentships for artizans in the Art Schools at South Kensington.

The necessity which has gradually made itself felt in the manufacturing towns of the United Kingdom for encouraging the study of science in its application to industries, by those who intend to devote themselves to some branch of manufacture or trade, has led to the establishment in about twenty-five towns in England and Scotland, and in two or three in Ireland, of colleges of science corresponding more or less to the Continental polytechnic schools, and accomplishing important work in training students in the different branches of science in their applications to manufactures and the arts. A number of these, such as the Owen's College, Manchester, the Yorkshire College, at Leeds, the Glasgow and Bradford Technical Colleges, the Firth College at Sheffield, and the Mason's College at Birmingham, have established a high reputation as schools where science in its applications to productive industries is most efficiently taught and importantly advanced.

The wealthier of the City Companies, some of which had long been identified with important educational establishments, associated themselves with the Corporation of the City of London nearly ten years ago to establish an organisation for the advancement of technical education, which has already carried out most important work. The Society of Arts, which initiated the system of examinations, afterwards so successfully developed by the Science and Art Department, set on foot and conducted for several years examinations of artizans in a few branches of technology. This useful work was relinquished in 1879 to the City and Guilds' Institute, and its extension since that period has been most satisfactory. The number of candidates then presenting themselves was 202, distributed over twenty-three centres where examinations were held, four years afterwards (1883) the number presenting themselves for examinations was 2397, and last year they amounted to 4764. The centres where examinations are held have been increased to 186, and the number of subjects dealt with, from thirteen to forty-eight. The beneficial influence exercised by these examinations upon the development and extension of technical instruction in the manufacturing districts throughout the country is already very marked. The adoption of the system, originated by the Science and Art Department, of contributing to the payment of teachers in proportion to the successes attained by their pupils, is operating most succesfully in promoting the establishment and extension of classes for instruction in technical subjects, in connexion with Mechanics' Institutes and other educational establishments in various centres of industry. In 1884, the number of classes in different parts of the country and metropolis which are connected with the examinations of the Institute was 262, having 6395 students, and this year the number of classes has risen to 357, and that of students to 8500.

The Technical College at Finsbury was the first great practical outcome of the efforts made by the City and Guilds' Institute to supplement existing educational machinery, by the creation of techno-

logical and trade schools in the metropolis, and the results, in regard to number and success of students at the day and evening schools of that important establishment, have afforded conclusive demonstration of the benefits which it is already conferring upon young workers who, with scanty means at their command, are earnest in their desire to train themselves thoroughly for the successful pursuit of industries and trades. The evening courses of instruction are especially valuable to such members of the artizan classes as desire, at the close of their daily labour, to devote time to the acquisition of scientific or artistic knowledge. The system of evening classes, which was pursued, in the first instance, at King's College and one or two other metropolitan schools, was most successfully developed by the Science and Art Department, and, being now supplemented by the important work accomplished at Finsbury College, is really, in point of organisation, in advance of similar work done in other countries.

Another department of the City and Guilds' Institute, of a somewhat different character, but akin to that of the Finsbury College in the objects desired to be achieved by it, is the South London School of Technical Art, which is also doing very useful work, while the chief or central Institution for Technical Education, which commenced its operations about three years ago, if it but continue to be developed in accordance with the carefully matured scheme which received the approval of the City and Guilds' Council, and with that judicious liberality which has been displayed in the design and arrangement of the building, bids fair to become the Industrial University of the Empire.

As one of the first students of that College of Chemistry which became part-parent of our present Normal Schools of Science, and the creation of which (forty-two years ago) constituted not the least important of the many services rendered towards the advancement of scientific education in this country by His Royal Highness the Prince Consort, most vividly I remember the struggling years of early existence of that half-starved but vigorous offspring of the great school of Liebig, born in a strangely unsympathetic land in the days when the student of science in this country still met on all sides that pride of old England, the practical man, enquiring of him complacently: *cui bono; quo bono?* That ardent lover of research and instruction, the enthusiastic and dauntless disciple of Liebig—my old master—Hofmann, loyally supported through all discouragement, and in the severest straits, by a small band of believers in the power of scientific research to make for itself an enduring home in this country, succeeded in very few years in developing a prosperous school of chemistry which soon made its influence felt upon British industry; and it is not credible that less important achievements should be accomplished, and less speedily, in days when the inseparable connection of science with practice has become thoroughly recognised, by an Institution created, and launched under most auspicious circumstances, by those powerful representatives of the commercial and industrial

prosperity of the Empire, who, before all others, must realise the vital necessity for ceaseless exertions, even for much self-sacrifice in the immediate present, to recover our lost ground in the Dominions of Industry.

It has been already demonstrated by the rapid increase which has taken place in the number of young men who, qualified by their preliminary education for admission as matriculated students, go through the complete curriculum of the Central Institute, that the combination of advanced scientific instruction with practical training which that course of study involves, will be much sought after by young men whose preliminary education has qualified them for admission, and whose probable future career will be interwoven with the advancement of one or other of the great industries of our country. But, one of the most important functions of the Central Technical College should consist in the thorough training of teachers of applied science. The statistics furnished by the technological examinations show that, while their successful organisation has led to the establishment of classes of instruction, supplementary to the general science teaching in every large manufacturing centre, the increase in the number of candidates examined has been accompanied by an increase in the percentage of failures to pass the examinations, and that the supply of a serious deficiency in competent teachers was essential to a radical improvement in technical education. The work of the City and Guilds' Institute in this direction has already been well begun, and it is in the furtherance of this, by the organisation of arrangements for facilitating the attendance of science teachers for sufficient periods at the Central Institute, or at more accessible provincial technical colleges, that the Imperial Institute may hope to do good work.

Without taking any direct part in the duty of education, it is contemplated that the Imperial Institute will actively assist in the thorough organisation of technical instruction, and its maintenance on a footing, at least of equality, with that provided in other countries, by the system of intercommunication which it will establish and maintain between technical and science schools; by the distribution of information relating to the progress of technical education abroad, to the progressive development of industries, and the requirements of those who intend to pursue them; by the provision of resources in the way of material for experimental work, and illustrations of new industrial achievements, and by a variety of other means.

The provision of facilities to teachers in elementary schools to improve their knowledge of science and their power of imparting information of an elementary character to the young, with the aid of simple practical demonstrations of scientific principles involved in the proceedings of daily life, constitutes another direction in which important progress may be made towards establishing that continuity between elementary and advanced education which is so well developed on the Continent. The organisation of facilities, combined

with material aid, to be provided to young artizans who shall afford some legitimate evidence of superior natural intelligence and a striving after self-improvement, to enable them to abandon for a time the duty of bread-winning, and to work at one or other of the technical schools in London or the provincial centres, will be another object to which the resources of the Imperial Institute should be applied very beneficially. Not only will the intelligent workman's knowledge of the fundamental principles of his craft or trade be thereby promoted; his association in work and study with others who are pursuing the acquisition of knowledge in different directions, which at first seem to him alien to his personal pursuits and tastes, but come in time to acquire interest or importance in his eyes, will bring home to him the advantages of a wider and more comprehensive scope of instruction, and the enlargement of his views regarding the value and pleasure of knowledge will, in turn, exercise a favourable influence in the same direction upon those with whom he afterwards comes into contact. The cramping influence which the great subdivision of labour, resulting from the development of mechanical, physical, and chemical science, is calculated to favour, must thus become counteracted, and the workman will realise, that if he is to rise above the level of the ordinary skilled labourer, mere dexterity in the particular branch of that trade which he has made his calling must be supplemented by an acquaintance with its cognate branches, by some knowledge of the principles which underlie his work, and by some familiarity with the trades allied to his calling.

The importance of bringing technical instruction within the reach of the needy scholars of the lower middle class need not be dwelt upon, and there can be no question that one of the most powerful means of promoting the extension of technical education will be the well organised administration of a really comprehensive system of scholarships, to be judiciously utilised in connection with the well-established colleges and schools of science and technics throughout the country, in such proportions as to meet local requirements and changing conditions. That a good foundation for such a system of scholarships is likely ere long to emanate from the resources of the Royal Commission of 1851, has already been officially indicated in one of its reports; may we not also hope that many will be found in our Empire ready to follow the example of the late Sir Joseph Whitworth, and to act in emulation of the patriotism of those men who, by munificent donations or endowments in aid of the work of bringing industrial education within the reach of all classes in the United States, have helped to place our Cousins in the position to hold their own and aspire to victory, in the war of industry? The thoroughly representative character which it is intended to maintain for the governing body of the Imperial Institute, will secure the wise administration by it of funds of this kind, dedicated to the extension and perfection of national establishments for technical education, and to the encouragement of its pursuit, in the ways above indicated, by

those whose circumstances would otherwise prevent them from enjoying the advantages secured to their fellow-workers in other countries. Several other directions readily suggest themselves in which the judicious administration of resources in aid of the technical training of eligible men of the artizan class could well form part of the organised work of the Imperial Institute.

By the establishment of an Education branch of the Intelligence Department, which will form a very prominent section of the Imperial Institute, the working of the colleges and schools of applied science in all parts of the United Kingdom will be harmonised and assisted, and the information continuously collected from all countries relating to educational work and the application of the sciences to industrial purposes and the arts will be systematically distributed. A well-organised Enquiry Department will furnish to students coming to Great Britain from the Colonies, Dependencies and India the requisite information and advice to aid them in selecting their place of work and their temporary home, and in various other ways. The collections of natural products of the Colonies and India, maintained up to the day by additions and renewals at the central establishment of the Institute, will be of great value to students in the immediately adjacent educational Institutions, and will moreover be made subservient to the purposes of provincial industrial colleges by the distribution of thoroughly descriptive reference catalogues, and of specimens. Supplies of natural products from the Colonies, India, or from other Countries, which are either new or have been but imperfectly studied, will be maintained, so that material may be readily provided to the worker in science or the manufacturer, either for scientific investigation or for purposes of technical experiment.

The existence of those collections and of all information relating to them, as well as of the libraries of technology, inventions, commerce and applied geography, in immediate proximity to the Government museums of science and inventions, art, and natural history, to the Normal School of Science, and to the Central Technical Institute, present advantages so obvious as to merit some fair consideration by those who have declined to recognise any reason in favour of the establishment of the Imperial Institute at South Kensington.

In the powerful public representations which have of late been made on the imperative necessity for the greater dissemination and thorough organisation of industrial education, the importance of a radical improvement in commercial education, as distinguished from what is comprehended under the head of technical training, has scarcely received that prominence which it merits. It is true that, in some of our colleges, there are courses of instruction framed with more especial reference to the requirements of those who propose to enter into mercantile houses, or in other ways to devote themselves to commercial pursuits; but as a rule the mercantile employés, embraced under the comprehensive title of clerks, begin their careers in life but

ill prepared to be more than mechanical labourers, and remain greatly dependent upon accident, or upon their desire for self-improvement which directs them in time to particular lines of study, for their prospects of future success in commercial life.

This impressed itself strongly upon the Royal Commission on the Depression of Trade and Industry, who state as the result of evidence collected by them that our deficiency in the matter of education as compared with some of our foreign competitors relates "not only to what is usually called technical education, but also to the ordinary commercial education which is required in mercantile houses." The ordinary clerk in a merchant's office is too often made to feel his inferiority to his German colleague, not merely in regard to his lamentable deficiency in the knowledge of languages, but in respect to almost every branch of knowledge bearing upon the intelligent performance of his daily work and upon his prospect of advancement in one or other branch of a mercantile house. The preliminary training for commercial life on the Continent is far more comprehensive, practical and systematic than that which is attainable in this country, and the student of commerce abroad has, afterwards, opportunities for obtaining a high scientific and practical training at distinct branches of the polytechnic schools and in establishments analogous to the technical colleges, such as the High Schools of Commerce in Paris, Antwerp, and Vienna.

It will be well within the scope of the Imperial Institute as an organisation for the advancement of industry and commerce, to promote a systematic improvement and organisation of commercial education by measures analogous to those which it will bring to bear upon the advancement of industrial education.

The very scant recognition which the great cause of technical education has hitherto received at the hands of our administrators has, at any rate, the good effect of rousing and stimulating that power of self-help which has been the foundation of many achievements of greatest pride to the Nation, and we may look with confidence to the united exertions of the people of this country, through the medium of the representative organisation which they are now founding, for the early development of a comprehensive national system of technical education, of the nature foreshadowed not long since by Lord Hartington, in that important address which has raised bright hopes in the hearts of the Apostles of education.

In some of the views which have been of late put forward regarding the possible scope of the Imperial Institute, the antagonism which has been raised and fostered against its location in the vicinity of some of our National Establishments most intimately connected with the educational advancement of the Empire, has developed a tendency to circumscribe its future sphere of usefulness, and to place its functions as a great establishment of reference and resort for the commercial man in the chief foreground. I have endeavoured to indicate directions in which its relations to the Colonies and India,

to the great industries of the country, and to the advancement of technical and commercial education, cannot fail to be at least as important as its immediate connection with the wants of the commercial section of the community, and those are most certainly quite independent of the particular locality in which it may be placed, excepting in so far as the command of ample space, and the advantages to be derived from juxta-position with the great National establishments to which I have referred, is concerned. At the same time, there is not one of the directions in which the development of the resources and activity of the Institute has been thus far indicated, which has not an immediate and important bearing upon the advancement of the commerce of the Empire. There are, however, special functions to be fulfilled by the Institute, which are most immediately connected alike with the great commercial work of the City of London and with that of the provincial centres of commerce. The provision, in very central and readily accessible positions, of commercial museums or collections of natural or import products, and of export products of different nations, combined with comprehensive sample-rooms and facilities for the business of inspection or of commercial, chemical or physical examination, is a work in which the Institute should lend most important aid. The system of correspondence with all parts of the Empire which it will develop and maintain will enable it to collect, and form a central depot of, natural products from which local commercial museums can be supplied with complete, thoroughly classified economic collections, and with representative samples of all that, from time to time, is new in the way of natural products from the Colonies and Dependencies, from India, and from other countries. In combination with this organisation, the distribution, to commercial centres, of information acquired by a central department of commercial geography will constitute an important feature in the work of the Institute, bearing immediately upon the interests of the merchant at home, in the Colonies, and in India.

The formation of specially commercial institutions, of which enquiry offices, museums, and sample rooms with their accessories, will form a leading feature, and which will supply a want long since provided for by the Nations with whom we compete commercially, is already in contemplation in the Cities of London and Newcastle; other great commercial centres will also doubtless speedily take steps to provide accommodation for similar offshoots from the central collections of the Institute. So far as the Indian Empire is concerned, the organisation of correspondence by provincial committees which already exists in connection with economic and geological museums established in the several Presidencies, affords facilities for the speedy elaboration of the contemplated system of correspondence in connection with the Institute, and the establishment of similar organisations in the different Colonies will, it is hoped, be heartily entered upon and speedily developed.

The system of correspondence to which I have more than once alluded in indicating some of the work of the Institute, in relation to technical education and industry, and which will form a most important part of the main groundwork of its organisation, is not in the least theoretical in its character. Its possible development has suggested itself to many who have given thought to the future sphere of action of the Institute in connection with commerce and industry; to myself, who for many years have been, from time to time, officially cognizant of the work performed by what are called the Intelligence Departments of the Ministries of War abroad and at home, the direct and valuable bearing of such a system upon the work of the Institute, suggested itself as soon as I gave thought to the possible future of this great conception, and to Major Fitzgerald Law belongs the credit of suggesting that the well-tried machinery of the War Office Intelligence Department should serve as a guide for the elaboration of a Commercial Intelligence Department. This Department, which will, it is hoped ere long commence its operations by establishing relations with the chief Colonies and India, will be in constant communication with the Enquiry Offices to be attached to the local commercial establishments and to other provincial representations of the work of the Institute, systematically distributing among them the commercial information and statistics continually collected. It will be equally valuable to the Colonies and India by bringing their requirements thoroughly to the knowledge of the business men in the United Kingdom, and by maintaining that close touch and sympathy between them and the people at home which will tend to a true federation of all parts of the Empire.

In no more important direction is this system destined to do useful work than in the organisation of emigration, not only of labour, but also of capital. The establishment of emigration enquiry offices at provincial centres in connection with a central department at the Institute, will be of great service to the intending emigrant, by placing within his reach the power of acquiring indispensable information and advice, and by facilitating his attainment of the special knowledge or training calculated to advance his prospects in the new home of his choice. Similarly, the capitalist may be assisted in discovering new channels for enterprise in distant portions of the Empire, the resources of which are awaiting development by the judicious application of capital and by the particular class of emigration which its devotion to public works or manufacturing enterprise in the Colonies would carry with it. The extent to which the State may aid in the organisation of systematic emigration, and the best mode in which it may, without burden to the Country, promote the execution of such public works in the Colonies as will open up their Dominions to commerce and at the same time encourage the particular class of emigration most advantageous to the Colonies themselves, are subjects of great present interest; but, in whatever way these important questions may be grappled with, such an or-

ganisation as the Institute should supply, cannot fail to accelerate the establishment of emigration upon a sound and systematic footing, and to co-operate very beneficially in directing private enterprise into the channels best calculated to advance the mutual interests of the capitalists and the Colonies.

I have already indicated that it is not only in connection with purely commercial matters that the Intelligence Department of the Institute will occupy itself. The prospects of its value to the Colonies and to India in promoting the development of their natural resources and the cultivation of new fields for commercial and industrial activity are well illustrated by the valuable work which has been accomplished upon similar lines by the admirably directed organization at Kew.

By the systematic collection and distribution of information relating to industries and to education from all countries which compete with ourselves in the struggle for supremacy in intellectual and industrial development, the Institute will most importantly contribute to the maintenance of intimate relationship and co-operation between educational, industrial, and commercial centres, between the labourer in science and the sources through which his work becomes instrumental in advancing national prosperity; between the Colonies and the Mother-Country, between ourselves and all Races included in the vast Empire of Her Majesty.

In conclusion, I venture to express the belief that the organisation which the Imperial Institute will have the power of developing, with a wisely constituted governing body at its head, may accomplish, and at no distant date, most useful work, which has been already publicly indicated as destined to have an immediate bearing upon the federation of England and her Colonies. Professor Huxley, in his last Presidential Address to the Royal Society, uttered most suggestive words, indicative of the value and the possibility of a *scientific* federation of all English speaking Peoples; and this subject is now receiving the careful consideration of that Society. It is firmly believed by leading men of science, that such a federation of at any rate the Colonies and Dependencies with us will be brought about, and it is in harmony with that belief that the Imperial Institute should be expected, through its organisation, to afford important aid in the application of the principle of federation to the geological and topographical survey of the Colonies, in the establishment of a system of interchange of meteorological and scientific observations, and in the promotion, in various ways, of thorough co-operation between particular Colonies or groups of Colonies, for applying the results of scientific research to the mutual development of their natural resources.

It may be that the programme of which I have given a very imperfect exposition, as indicative of the work which the Imperial Institute may be called upon to accomplish, will be regarded as almost too ambitious in its scope for practical fulfilment. The outline

of this programme has been drawn by a combination of abler hands than mine; I have but ventured to sketch in some of the details as they have presented themselves to my mind, and to the minds of others who have given thought to this great subject; but I dare to have faith in its realisation, and to believe that, if the work be taken in hand systematically and progressively, the nucleus being first thoroughly established from which fresh lines of departure will successively emanate, the Imperial Institute is destined to become a glory of the land. And, as one whose mission it has been, through many years of arduous work, to assist in a humble way in the application of the resources of some branches of science to the maintenance of the country's power to defend its rights and to hold its own, I may perhaps be pardoned for my presumption in giving expression to the firm belief that, by the secure foundation and careful development of this great undertaking, and by its wise direction by a Government truly representative of its Founders—all Nations and Classes composing the Empire—there will be secured in it one of the most important future Defences of the Queen's Dominions; one of the most powerful instruments for the maintenance of the unity, the strength, and the prosperity of Her Realms.

At the conclusion of the address, the Prince of Wales said:—

"Ladies and Gentlemen,—Having had the honour of occupying the Chair this evening, I think we ought not to separate without my expressing, on your part as well as on my own, our deep sense of gratitude for the interesting and exhaustive lecture which Sir Frederick Abel has just given us. You are well aware of the great interest I take in this Imperial Institute, as I am anxious that it shall be a memorial of the Jubilee of Her Majesty the Queen, and at the same time a memorial which shall be of great use to this country, and cement still further the good feeling which I trust has always existed in the Mother Country towards our Colonies and India. Sir Frederick Abel has, I think, touched on all the salient points with regard to the Imperial Institute, so that it will be needless for me to say anything further. But I thank him very much again for having given us this lecture this evening, and I feel sure that those who have been here and who did not feel conversant with the subject, will carry away with them a very clear understanding of what the objects of the Institute are." (Cheers.)

THE BRITISH COLONIES.

ILLUSTRATIONS OF THEIR DEVELOPMENT DURING THE QUEEN'S REIGN.

IMPORTS AND EXPORTS.

		Imports. £	Exports. £
American Dependencies	1837	5,200,000	5,000,000
	1885	25,700,000	21,500,000
Australasia	1837	1,500,000	1,300,000
	1885	63,500,000	52,000,000
Africa	1837	2,000,000	1,500,000
	1885	10,000,000	12,000,000

All the Imports and Exports taken together were ELEVEN TIMES larger in 1885 than they were in 1837.

British Shipping Trade with Colonies	1837	3,700,000 tons.
	1885	56,600,000 "
British Export to Colonies	1837	11,300,000*l.*
	1885	54,500,000*l.*

POPULATION.

Of all the Colonies existing in 1837	1837	4,204,700
	1881	12,753,277*
Of all the Colonies in 1881		15,763,072*

* These numbers must have considerably increased since 1881.

RATE OF INCREASE FROM 1837 TO 1881.

In European Colonies	SLIGHT.
In Ceylon	TWICE as large as it was.
In the Great Asiatic Colonies	About the SAME.
In the Cape of Good Hope	EIGHT TIMES as large as it was.
In Canada	THREE TIMES as large as it was.
In the West Indies	NOT quite TWICE as large as it was.
In Australia	Nearly TWELVE times as large as it was.

(Compiled in the

	How and when Acquired.		Area.	
			Square Miles.	
British Isles	..	..		120,832
Indian Empire (including Burmah)	..	1757–1858		1,574,516
Dominion of Canada:—				
Quebec	Conquest, Treaty Cession	1759–63		
Ontario				
New Brunswick ..	Treaty Cession	1763		
Nova Scotia	Conquest, Treaty Cession	1627–1713		3,470,392
British Columbia ..	Transfer to Crown ..	1858		
Manitoba	Settlement	1813		
North-West Territories	Charter to Company ..	1670		
Prince Edward's Island	Conquest	1756–63		
Newfoundland	Settlement, Treaty Cession	1550–1713		40,200
Australasia:—				
New South Wales ..	Settlement	1787	311,098	
Victoria	Settlement	1834	87,884	
South Australia	Settlement	1836	903,690	
Queensland	Settlement	1824	668,497	
Western Australia ..	Settlement	1826	1,060,000	
Tasmania	Settlement	1803	26,215	
New Zealand	Purchase	1840	104,458	
Fiji	Cession from Natives ..	1874	7,740	
New Guinea	Annexation	1884	86,360	
				3,255,942
South Africa:—				
Cape of Good Hope ..	Treaty Cession (finally)	1815	219,700	
Bechuanaland	.. ..	1885	185,000	
Natal	Annexation	1843	18,750	
				423,450
St. Helena	Conquest	1673		45
Ascension	Annexation	1815		37
Ceylon	Treaty Cession	1801		25,365
Mauritius	Conquest and Cession ..	1810–14		713
Straits Settlements ..	Treaty Cession	1785–1824		1,472
Hong Kong	Treaty Cession	1841		30
Port Hamilton	.. ..	1884		5
British North Borneo ..	Cession to Company ..	1877		30,000
Labuan	Treaty Cession	1847		30
British Guiana	Conquest and Cession ..	1803–14		109,000
West Indies:—				
Jamaica	Conquest	1655	4,362	
Trinidad	Conquest	1797	1,754	
Windward Islands	Cession	1783	784	
Leeward Islands ..	.. ..	..	665	
Bahamas	Settlement	1629	5,390	
				12,955
Bermudas	Settlement	1612		19
British Honduras ..	Conquest	1798		6,400
West Africa:—				
Sierra Leone	Transfer from Company	1807	468	
Gambia	.. ..	..	69	
Gold Coast	Conquest and Cession ..	1663–1871	18,784	
Lagos	Cession	1861	1,069	
				20,390
Gibraltar	Conquest	1704		2
Malta	Treaty Cession	1814		119
Cyprus	Convention with Turkey	1878		3,584
Heligoland	Treaty Cession	1814		1
Falkland Islands	Treaty Cession	1770		6,500
				9,101,999

Population.		Imports.		Exports.	
	35,241,482	Total. £390,018,569	From Colonies. £95,812,911	Total. £295,967,583	To Colonies. £88,303,634
	253,982,595	Total. 68,156,654	From British Isles. 49,711,562	Total. 89,098,427	To British Isles. 36,984,034
	4,324,810	23,917,200	8,921,510	18,782,156	8,986,897
	179,509	1,682,457	642,528	1,368,153	322,527
921,268		22,826,985	11,423,047	18,251,506	7,683,886
961,276		19,201,633	9,149,076	16,050,465	7,745,415
312,781		5,749,353	2,983,296	6,623,704	4,081,864
309,913		6,381,976	2,520,863	4,673,864	1,715,391
31,700		521,167	222,940	405,693	279,660
130,541		1,656,118	642,102	1,475,857	359,708
564,304		7,663,888	4,934,493	7,091,667	5,158,078
128,614		434,522	..	345,344	35,542
135,000		..	..	..	..
	3,495,397				
1,249,824		5,260,697	4,023,819	7,031,744	6,602,193
..		..	..	..	..
424,495		1,675,850	1,310,452	957,918	721,190
	1,674,319				
	5,024	63,786	27,931	23,406	1,164
	200	..	..	..	..
	2,763,984	4,811,451	1,315,345	3,161,262	1,852,829
	377,373	2,963,152	692,430	3,941,757	508,331
	540,000	18,676,766	4,282,920	17,260,138	3,845,362
	160,402	4,000,000	3,218,946	2,000,000	1,052,302
	2,000	..	..	..	..
	150,000	96,282	..	52,551	..
	6,298	84,869	1,554	85,741	..
	264,061	1,999,448	1,099,504	2,322,032	1,777,376
585,536		1,595,262	910,194	1,518,024	643,971
153,128		3,083,870	887,011	2,769,727	863,290
311,413		1,611,483	670,955	1,834,388	797,194
119,546		476,457	207,637	466,759	160,903
43,521		181,494	37,329	122,351	35,771
	1,213,144				
	13,948	283,440	75,416	88,622	2,557
	27,452	237,538	127,602	317,449	205,032
60,546		455,424	323,572	377,055	156,730
14,150		212,122	87,099	199,483	18,753
408,070		537,339	403,788	467,228	330,997
75,270		538,221	338,318	672,414	249,794
	558,036				
	18,381	..	..	..	..
	149,782	13,343,789	122,899	12,908,492	3,120,319
	186,173	304,375	..	287,521	..
	2,001	..	..	..	..
	1,553	67,848	60,962	101,338	98,468
	305,337,924	£220,752,916	£111,377,100	£223,134,236	£96,397,528

WEEKLY EVENING MEETING,

Friday, April 29, 1887.

Henry Pollock, Esq. Treasurer and Vice-President, in the Chair.

Professor H. S. Hele Shaw.

The Rolling Contact of Bodies.

When two solid bodies roll upon each other, points in the surface of one successively come into contact with corresponding points in the surface of the other in a way which differs essentially from that which occurs in sliding contact, and it is the nature of this rolling-contact that the lecturer proposed to discuss in an experimental manner.

In the first place, it is well to understand clearly the nature of the relative motion of the two points which come into contact when the surfaces are such that no appreciable distortion of them takes place, and for this purpose one of the two bodies must be at rest. These may respectively be taken as the plane surface of the ground and a circular disk rolling upon it. An approximate representation of this motion is given by the end of the spokes of a wheel without its tyre. In this case it is seen that a point of the rolling body, when it is just coming into contact with the fixed surface, does so in a direction at right angles to the surface at rest, and also leaves it in the same direction. This action is very similar in kind to that which occurs with the continuous circle formed by the tyre. The path of a point in the rim can be drawn in a way visible to the audience by means of a piece of apparatus consisting of two circular glass plates held together by a hollow brass spindle in which slides an arm carrying a brush. The brush traces the well-known cycloid, of which the only portion now to be considered is that where it directly approaches the surface beneath. This part is perpendicular to that surface, and when epicycloids are drawn, by rolling the disk upon the arc of a circle, the same fact is brought out.

One body may, however, not merely roll upon another, and a normal pressure be exerted, but they may exert a tangential force upon each other. It is convenient to keep these two cases separate; examples of them being respectively the wheels of a railway carriage and those of the locomotive which draws it along. It is to be noted that the object in the former case is to permit one body to move relatively to another without permitting sliding contact of their surfaces, whilst, in the latter case, in addition to this, the object is to obtain such motion. There are, however, many cases in which it is merely the motion of a body about one point which is required, such

as when motion is transmitted from the edge of one rotating disk to another, and then this distinction still more closely holds, as the normal pressure is only obtained so as to insure the necessary tangential resistance. Thus the objects of rolling motion may be classed as being—

(1) To *allow* the relative motion of one body to another with which it is in contact without permitting relative motion of that part of their surfaces in actual contact.

(2) To *obtain* the relative motion of such parts of the surfaces of bodies as are not in contact by means of statical contact of the parts which are.

The lecturer then proceeded to consider the practical proofs of the smallness of the resistance to rolling in cases where the distortion of the surfaces in contact is very small, as illustrated by the small tractive force required for heavy bodies properly mounted on wheels or on roller-bearings; mentioning the case of a 12-horse-power engine, the shaft of which continued to rotate for three-quarters of an hour after the motive power was withdrawn; and another case, of a turn-table weighing 14 tons, which was kept in motion by a weight of $3\frac{1}{2}$ pounds acting upon it by means of a cord passing over a pulley. The small distortion of such surfaces when transmitting motion requiring expenditure of energy to maintain, was next made clear by giving certain facts as to the accuracy with which one surface was developed or measured out upon another. An account was given of experiments made with apparatus specially prepared by the lecturer to investigate this point. This apparatus consisted of two accurately turned brass disks properly mounted upon a frame, and the relative positions of these disks could be interchanged so that any minute differences in their peripheries could be detected. The experiments, which were very difficult to carry out accurately, showed that under the best circumstances, motion with an error of only 1 in 300,000 of the distance passed over could be obtained. This accurate measuring out of the surfaces one upon another was employed in various ways for purposes of measurement, and these, by means of models and diagrams, were briefly explained.

Although the foregoing facts prove that, under suitable conditions, distortion at the points of contact is very small, yet some resistance at these points *always* occurs, because no bodies are perfectly hard; and the nature of this distortion and consequent resistance was next discussed.

The explanation of the resistance opposed by a soft surface to a hard body rolling upon it, as first given by Prof. Osborne Reynolds, was applied by the lecturer to account for a very remarkable effect produced in the disk, globe, and cylinder integrator of Prof. James Thomson. This effect, which was the turning of the cylinder when the sphere was rolled along it in a horizontal direction, was reproduced by means of a large model. The action of a soft body rolling upon a hard surface was next considered, with the result of showing

that the same reasoning would not account for the turning of the cylinder in the same direction as before with the above model, and the lecturer then proceeded, by means of diagrams, to offer an explanation of this and other phenomena. The various effects obtained with bodies of different relative degrees of hardness were discussed at length, but figures would be needed to make these points clear. Finally, an explanation was given of the cause of an error which always appeared in a certain important class of integrators caused by the slipping of the edge of a disk over a surface on which it rolled in circumstances under which it had apparently never been suspected that slipping did actually take place. This the lecturer had been enabled to discover and measure by means of a special piece of apparatus, a model of which was exhibited and the effects shown by its means.

The facts and reasoning, which were given in the lecture, all related to the rolling contact of bodies, and the lecturer ventured to think that, imperfect as the treatment of the subject had been, it was one of such importance, not merely from the point of view of the practical applications he had mentioned, but in its scientific aspect, dealing as it did from a novel point of view with the nature and properties of solid bodies, as to be worthy of being thus brought before the Royal Institution.

ANNUAL MEETING,

Monday, May 2, 1887.

Sir William Bowman, Bart. LL.D. F.R.S. Manager and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1886, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 83,000*l.* entirely derived from the Contributions and Donations of the Members.

Forty-eight new Members paid their Admission Fees in 1886.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1886.

The Books and Pamphlets presented in 1886 amounted to about 288 volumes, making, with 443 volumes (including Periodicals bound) purchased by the Managers, a total of 731 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. D.C.L. LL.D.
Treasurer—Henry Pollock, Esq.
Secretary—Sir Frederick Bramwell, D.C.L. F.R.S. M. Inst. C.E.

Managers.

oseph Brown, Esq. Q.C.
ir James Crichton Browne, M.D. LL.D. F.R.S.
he Earl of Crawford and Balcarres, F.R.S. F.R.A.S.
rank Crisp, Esq. LL.B. B.A. F.L.S.
'arren de la Rue, Esq. M.A. D.C.L. F.R.S.
enry Doulton, Esq.
ohn Hall Gladstone, Esq. Ph.D. F.R.S.
r William Withey Gull, Bart. M.D. D.C.L. F.R.S.
illiam Huggins, Esq. D.C.L. LL.D. F.R.S.
lfred Bray Kempe, Esq. M.A. F.R.S.
eorge Matthey, Esq. F.R.S. Assoc. Inst. C.E.
ne Right Hon. Earl Percy.
r Frederick Pollock, Bart. M.A.
illiam Henry Preece, Esq. F.R.S. M. Inst. C.E.
ward Woods, Esq. Pres. Inst. C.E.

Visitors.

Forster F. Arbuthnot, Esq.
Shelford Bidwell, Esq. M.A. F.R.S.
John Birkett, Esq. F.L.S. F.R.C.S.
Michael Carteighe, Esq. F.C.S.
The Very Rev. Dean Church, M.A.
Edwin Cutler, Esq.
James Farmer, Esq.
Charles Hawksley, Esq. M. Inst. C.E.
David Edward Hughes, Esq. F.R.S.
John W. Miers, Esq.
Frederick Purdy, Esq. F.S.S.
Lachlan Mackintosh Rate, Esq. M.A.
William Chandler Roberts-Austen, Esq. F.R.S.
George John Romanes, Esq. M.A. LL.D. F.R.S.
James Wimshurst, Esq.

WEEKLY EVENING MEETING,

Friday, May 6, 1887.

The Right Hon. EARL PERCY, Manager and Vice-President, in the Chair.

T. LAUDER BRUNTON, M.D. Sc.D. F.R.S.

The Element of Truth in Popular Beliefs.

THE common saying "Seeing is believing" gives a clue to the origin of many popular delusions, for the evidence of our eyes is by no means to be trusted, and unless corrected by the observations derived from other senses will often prove deceptive. Some popular beliefs are correct in regard to fact, but erroneous in regard to interpretation. Some others, which in their present form are absurd, are the survivals or modifications of other beliefs which were true.

In endeavouring to discover the element of truth in any belief we may be aided by tracing its history backwards in time, or by comparing it with allied forms of belief in different places.

As an example of the historical method we may take the belief that horse-flesh is unfit for food, a delusion which arose from the circumstance that horse-flesh was unfit for Christian food, inasmuch as the horse was sacred to Odin, and eating its flesh was a sign of Paganism.

As an illustration of the comparative method we may take the belief that a person cannot die if any door in the house be locked. Other forms of the belief are that a person cannot die as long as the doors or windows of the room in which he is lying are closed, and observation enables us to ascertain that this is due to the fact that the room is thus kept warm, and life therefore prolonged.

The belief that disease may be cured by hanging up rags in a sacred place may be connected by intermediate forms with the fact that infectious diseases may be conveyed from one to another by articles of clothing.

Some omens probably have an historical origin. Others depend on physical conditions, such as stumbling on leaving the threshold as an indication of coming misfortune. This may be regarded as simply an evidence of a deficiency in the motor power of the individual which may cause him to fail in an emergency.

Others again may be referred to indistinct sensations or subconscious conditions. Dreams are frequently influenced by the circumstances of the dreamer, either at the time or some days before,

and hallucinations as well as visions of ghosts and fairies may be regarded as forms of waking dreams.

The signs which were regarded in the Middle Ages as distinctive of witchcraft are now looked upon as symptoms of hysteria, and the condition of hysteria may perhaps be defined to be one in which impressions originating within the body itself tend to overpower those transmitted from without by the usual sensory channels.

The phenomena of thought reading and of the divining rod may in many cases be explained by the fact that sensory impressions may be received and may lead to action without rising into complete consciousness in the individual who receives them.

[T. L. B.]

GENERAL MONTHLY MEETING,

Monday, May 9, 1887.

The Right Hon. Earl Percy, Manager and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

The Earl of Crawford and Balcarres, F.R.S. F.R.A.S.
Warren de la Rue, Esq. M.A. D.C.L. F.R.S.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
The Right Hon. Earl Percy.
Sir Frederick Pollock, Bart. M.A.
Edward Woods, Esq. Pres. Inst. C.E.
Henry Pollock, Esq. Treasurer.
Sir Frederick Bramwell, D.C.L. F.R.S. M. Inst. C.E. Honorary Secretary.

John Donaldson, Esq. M. Inst. C.E.
Miss Mary Augusta Grant,
Mrs. Bayne Ranken,
Owen Roberts, Esq. M.A.
Mrs. Shore Smith,
Frederick Meadows White, Esq. Q.C.

were elected Members of the Royal Institution.

John Tyndall, Esq. D.C.L. LL.D. F.R.S. was elected Honorary Professor of Natural Philosophy.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. was elected Professor of Natural Philosophy.

The Special Thanks of the Members were returned for the following donations to the Fund for the Promotion of Experimental Research:—

William Henry Domville, Esq. £20.

The "Faraday Memorial" Committee } £212 6*s.* 1*d.* being the balance remaining after payment for the statue of Professor Faraday by Foley, and its pedestal, and a bust copied from the statue presented to the National Portrait Gallery.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Bernays, Albert J. Esq. M.R.I. (the Author)—Notes for Students in Chemistry. 6th Edition. 12mo. 1878.
British Architects, Royal Institute of—Proceedings, 1886–7, No. 13. 4to.
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General Guide to the British Museum (Natural History). 8vo. 1887.
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Editors—American Journal of Science for April, 1887. 8vo.
Analyst for April, 1887. 8vo.
Athenæum for April, 1887. 4to.
Chemical News for April, 1887. 4to.
Chemist and Druggist for April, 1887. 8vo.
Engineer for April, 1887. fol.
Engineering for April, 1887. fol.
Horological Journal for April, 1887. 8vo.
Industries for April, 1887. fol.
Iron for April, 1887. 4to.
Murray's Magazine for April, 1887. 8vo.
Nature for April, 1887. 4to.
Revue Scientifique for April, 1887. 4to.
Telegraphic Journal for April, 1887. 8vo.
Zoophilist for April, 1887. 4to.
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Franklin Institute—Journal, No. 736. 8vo. 1887.
Freshfield, Edwin, Esq. LL.D. V.P.S.A. M.R.I. (the Author)—Some Remarks on the Book of Records and History of the Parish of St. Stephen, Coleman Street. 4to. 1887.
Geographical Society, Royal—Proceedings, New Series, Vol. IX. No. 4. 8vo. 1887.
Geological Institute, Imperial, Vienna—Abhandlungen, Band XII. No. 4. 4to. 1886.
Jahrbuch, Band XXXVI. Heft 4. 8vo. 1887.
Verhandlungen, 1886, Nos. 13–18; 1887, No. 1. 8vo. 1886–7.
Hall and Co. Messrs. (the Publishers)—Sanskrit Vocabulary. 4to. 1885.
Harlem, Société Hollandaise des Sciences—Archives Neerlandaises, Tome XXI. Liv. 4. 8vo. 1887.
Hennessy, Henry, Esq. F.R.S. (the Author)—Problems in Mechanism regarding Trains of Pulleys, &c. (Royal Society Proc.) 8vo. 1887.

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Linnean Society—Journal, Nos. 127, 128, 148. 8vo. 1887.
Manchester Geological Society—Transactions, Vol. XIX. Parts 6, 7. 8vo. 1887.
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WEEKLY EVENING MEETING,

Friday, May 13, 1887.

Henry Pollock, Esq. Treasurer and Vice-President, in the Chair.

Professor J. S. Burdon-Sanderson, M.D. LL.D. F.R.S.

Some Electrical Fishes.

The lecture was divided into three parts, in the first of which a general description was given of the three most important electrical fish, viz. the Torpedo, or electrical ray; the electrical eel, of the rivers and lakes of South America; and the Malapterurus, of the Nile and Senegal. In the second part the lecturer discussed the anatomical character and morphological significance of the electrical organ in torpedo, and in the third its mode of action, with special reference to the recent investigations of Mr. Francis Gotch, Assistant in the Physiological Department at Oxford. The description given of the structure of the organ was also founded on new investigations by Prof. Ewart, of Edinburgh, who had been good enough to prepare drawings on glass suitable for projection on the screen, of his microscopical preparations. The first of these drawings showed a section of the already active electrical organ of a torpedo just born. It was seen to consist of a great number of tubular columns which extended from the upper (dorsal) to the lower (ventral) surface of the flattened body of the animal, which were as closely packed together as the cells of a honeycomb, each column being divided into very narrow compartments by nearly horizontal partitions of extremely fine membrane. It was next pointed out that although the whole organ is made up in the common torpedo of as many as 500 such columns (in some species many more), each column is in structure and in function an electrical organ of itself; and not only so, but that each of the fine membranous partitions or *plates* is an electromotive structure of which, notwithstanding its almost inconceivable tenuity, the two opposite surfaces are, when in activity, in different electrical states; so that, in consequence of their pile-like arrangement and their all acting in the same direction, the electromotive force excited by the whole column is, as in a voltaic battery, equal to the *sum* of the forces exerted by the many hundreds of plates of which it is composed.

It having thus been made evident that everything depended on the plates, the lecturer proceeded to explain their minute structure, for the investigation of which it was of course necessary to employ much

higher powers. The microscopical drawings which were thrown on the screen showed that each of the fine membranes which had been described consists of two different structures. Its upper surface presents a layer of apparent homogeneous material in which nuclei are distributed at intervals. This may be called the protoplasmic lamina. The under or ventral layer might be called the nerve lamina, for it is made up of the arborisations of the innumerable nervous filaments which spread themselves over the protoplasmic lamina on its under surface. As these filaments branch repeatedly as they approach their destination, their ultimate endings are among the smallest objects which can be distinguished under the microscope.

The electrical organ offers to the physiologist one of the most striking examples of that adaptation of structure to function which is universal among living beings. A single column of the organ of torpedo resembles in a very remarkable degree a voltaic pile, of which the plates are the elements, but it is a resemblance with a difference. The difference lies in this, that the organ is only a battery when it is waked into activity by a stimulus. This waking up or (to use the ordinary language of physiology) excitation, is derived from the animal's brain, which for the purpose has added to it a special electric lobe on each side, from which the enormous nerves, which are so richly supplied to the electrical organ, emanate. The use of this lobe is obviously not to produce electricity itself, but, at the will of the animal to set free the energy of the organ, i. e. of each of the many thousand plates of which it consists. Thus, of the two laminæ of each plate, the nervous and the protoplasmic, each represents a distinct function, the protoplasmic that of producing the required electromotive effect, the nervous, that of receiving from the brain and communicating to the protoplasm the impulse by which it is discharged.

In a former lecture it had been shown that all the ordinary physiological changes which occur at every moment of our existence in what Bichat called the organs of animal life, particularly in our nerves and muscles, are accompanied by electrical changes, and that although it is not yet possible to give any physical explanation of these changes, rapid progress is now being made in determining the laws of their association with the other physical concomitants of muscular and nervous action. As it is practically much more important to understand the physiology of muscle and nerve than that of the electrical organs of a few fish, the latter has been comparatively insufficiently studied. The purpose of the experiments made at Arcachon is to bring the phenomena of the electrical discharge or shock of torpedo and the physiology of its organ, into line with the already very accurately investigated phenomena of nerve and muscle. With reference to these last, certain very definite laws have been established, of which, perhaps, the most fundamental is that when functionally at rest, these structures exhibit no electromotive action. The structure must have been *previously* acted upon

by some external agency capable of exciting it. Another established fact is that the effect is of limited duration, and that for its development a certain time must elapse, which under similar conditions is always the same for the same structure. A third is that all kinds of excitants act in the same way, the effects differing in intensity, not in direction. In all these respects, and in others of less importance, the electrical plate agrees with muscle and nerve. Inasmuch, therefore, as we have met with a structure, of which the development of electrical action is the exclusive function, there seems to be good reason for the hope that by its investigation, a nearer approach may be made than has hitherto been possible to the central question—that of the reason why in all animal structures the transition from the inactive to the active state is, so far as our present knowledge teaches, always accompanied by electrical change.

The question why certain fish are endowed with so singular a means of offence and defence, which others allied to them zoologically do not possess, and above all, why some fish have electrical organs so small as to be useless, is as difficult to answer now as when Mr. Darwin wrote the 'Origin of Species.' The facts relating to the development of the organ, which teach us to regard it as, in some sense, a modified muscle, might suggest that the transition from muscle to organ was a gradual one determined by external conditions. But we are prevented from accepting any such suggestion by the consideration that an electrical organ only becomes advantageous to its possessor when it has acquired sufficient size to be used in the capture of prey, and that in all previous stages of transition it must be useless. Natural selection could not therefore determine the development of the electrical organ by modification of muscle. It is more reasonable to imagine that all fish, or at any rate certain families of fish, possess an undeveloped element of structure, of which the electrical organ is the manifestation. So that what we have to account for is not its presence in some exceptional cases, but its absence in the great majority.

The existence of such a tendency as this hypothesis supposes, would render it possible for natural selection to operate efficiently in bringing about the observed result.

[J. B. S.]

WEEKLY EVENING MEETING,

Friday, May 20, 1887.

Sir Frederick Bramwell, D.C.L. F.R.S. M.Inst.C.E. Honorary Secretary and Vice-President, in the Chair.

Benjamin Baker, Esq. M.Inst.C.E. *M.R.I.*

Bridging the Firth of Forth.

During the past four years many thousands of visitors from all parts of the United Kingdom, and, indeed, I may say from all parts of the world, have more or less carefully inspected the works now in progress under the superintendence of Sir John Fowler, the Engineer-in-Chief, and myself, for bridging the Firth of Forth. The present lecture is an imperfect attempt to convey to you, by description and illustration, some notion of the magnitude of the proportions and difficulties of construction of what is generally admitted to be one of the most important engineering works yet undertaken.

The Forth which "bridled the wild Highlander," and especially that part of it where the bridge crosses, should be well enough known to every reader of fiction, for it has been made the scene of many adventures. Mr. Louis Stevenson's thrilling story, "Kidnapped," will have been read by most of you; the hero of that story was kidnapped at the very spot where the bridge crosses, so I can describe the point of crossing in David Balfour's own words:—

"The Firth of Forth (as is very well known) narrows at this point, which makes a convenient ferry going north, and turns the upper reach into a land-locked haven for all manner of ships. Right in the midst of the narrows lies an island with some ruins; on the south shore they have built a pier for the service of the ferry, and at the end of the pier, on the other side of the road, and backed against a pretty garden of holly trees and hawthorns, I could see the building which they call the Hawes Inn." Such was the appearance of the spot 150 years ago. The middle pier of our bridge now rests on the island referred to, and the Hawes Inn flourishes too well, for being in the middle of our works its attractions prove irresistible to a large proportion of our 3500 workmen. The accident ward adjoins the pretty garden with hawthorns, and many dead and injured men have been carried there, who would have escaped had it not been for the whiskey of the Hawes Inn.

I would wish if possible to impress upon my hearers the exceptional size of the Forth Bridge, for even those who have visited the works and noted the enormous gaps to be spanned on each side of Inch Garvie, may yet have gone away without realising the magnitude of the Forth Bridge as compared with the largest railway bridges hitherto built. For the same reason that architects introduce human

figures in their drawings to give a scale to the buildings, do we require something at Queensferry to enable visitors to appreciate the size of the Forth Bridge. If we could transport one of the tubes of the great Britannia Bridge from the Menai Straits to the Forth, we should find it would span little more than one-fourth of the space to be spanned by each of the great Forth Bridge girders. To get an idea of the magnitude of the latter, stand in Piccadilly and look towards Buckingham Palace, and then consider that we have to span the entire distance across the Green Park, with a complicated steel structure weighing 15,000 tons, and to erect the same without the possibility of any intermediate pier or support. Consider also that our rail level will be as high above the sea as the top of the dome of the Albert Hall is above street level, and that the structure of our bridge will soar 200 feet yet above that level, or as high as the top of St. Paul's.

It is not on account of size only, that the Forth Bridge has excited so much general interest, but also because it is of a previously little known type. I will not say novel, for there is nothing new under the sun. It is a cantilever bridge. One of the first questions asked by the generality of visitors at the Forth is—Why do you call it a cantilever bridge? I admit that it is not a satisfactory name, and that it only expresses half the truth, but it is not easy to find a short and satisfactory name for the type. A cantilever is simply another name for a bracket, but the 1700 feet openings of the Forth are spanned by a compound structure consisting of two brackets or cantilevers and one central girder. Owing to the arched form of the underside of the bridge many persons hold the mistaken notion that the principle of construction is analogous to that of an arch. In preparing for this lecture the other day, I had to consider how best to make a general audience appreciate the true nature and direction of the stresses on the Forth Bridge, and after consultation with some of our engineers on the spot a living model of the structure was arranged as follows:—Two men sitting on chairs extended their arms and supported the same by grasping sticks butting against the chairs. This represented the two double cantilevers. The central girder was represented by a short stick slung from one arm of each man, and the anchorages by ropes extending from the other arms to a couple of piles of brick. When stresses are brought on this system by a load on the central girder, the men's arms and the anchorage ropes come into tension, and the sticks and chair legs into compression. In the Forth Bridge you have to imagine the chairs placed a third of a mile apart and the men's heads to be 360 feet above the ground. Their arms are represented by huge steel lattice members, and the sticks or props by steel tubes 12 feet in diameter and $1\frac{1}{4}$ inches thick.

I have evidence that even savages when bridging in primitive style a stream of more than ordinary width, have been driven to the adoption of the cantilever and central girder system as we were driven to it at the Forth. They would find the two cantilevers in

the projecting branches of a couple of trees on opposite sides of the river, and they would lash by grass ropes a central piece to the ends of their cantilevers and so form a bridge.

The best evidence of approval is imitation, and I am pleased to be able to tell you that since the first publication of the design for the Forth Bridge, practically every big bridge throughout the world has been built on the principle of that design and many others are in progress.

There are three main piers at the Forth known respectively as the Fife pier, the Inch Garvie pier, and the Queensferry pier, and upon each of these there are built huge cantilevers stretching both ways. The Fife pier stands between high and low-water mark, and is separated by a span of 1700 ft. from the Inch Garvie pier, which is partly founded upon a rocky island in mid-stream. Another span of 1700 ft. carries the bridge to the Queensferry pier, which is at the edge of the deep channel. The total length of the viaduct is about $1\frac{1}{2}$ miles, and this includes two spans of 1700 ft., two of 675 ft., being the shoreward ends of the cantilevers, and fifteen of 168 ft. Including piers, there is thus almost exactly 1 mile covered by the great cantilever spans and another $\frac{1}{2}$ mile of viaduct approach. The clear headway under the centre of the bridge is 152 ft. at high water, and the highest point of the bridge is 360 ft. above the same datum.

Each of the main piers includes four columns of masonry founded on the rock or boulder clay. Below low water the piers differ somewhat in character, according to the local conditions. At Inch Garvie two wrought-iron caissons, which might be likened to large tubs or buckets, 70 ft. in diameter and 50 ft. to 60 ft. high, were built on launching ways on the sloping southern foreshore of the Forth. The bottom of each caisson was set up 7 ft. above the cutting edge, and so constituted a chamber 70 ft. in diameter and 7 ft. high, capable of being filled at the proper time with compressed air to enable men to work as in a diving bell below the water of the Forth. The caisson, weighing about 470 tons, was launched and then taken to a berth alongside the Queensferry jetty, where a certain amount of concrete, brickwork, and staging was added, bringing the weight up to 2640 tons. A very strong and costly iron staging had previously been erected, alongside which the caisson was moored in correct position for sinking. Whilst the work described was proceeding, divers and labourers were engaged in making a level bed for the caisson to sit on. The 16 ft. slope in the rock bottom was levelled up by bags filled with sand or concrete. As soon as the weight of caisson and filling reached 3270 tons the caisson rested on the sand bags and floated no more. The high ledge of rock upon which the northern edge of the caisson rested was blasted away, holes being driven, by rock drills and otherwise, under the cutting edge, and about 6 in. beyond for the charges. After the men had gained a little experience in this work no difficulty was found in under-cutting the hard whinstone rock to allow the edge of the caison to sink, and, of course,

there was still less difficulty in removing the sand bags temporarily used to form a level bed. The interior rock was excavated as easily as on dry land, the whole of the 70 ft. diameter by 7 ft. high chamber being thoroughly lighted by electricity. Access was obtained through a vertical tube with an air lock at the top, and many visitors ventured to pass through this lock into the lighted chamber below, where the pressure at times was as high as 35 lbs. per square inch.

At *Queensferry* all four piers were founded on caissons identical in principle with those used for the deep Garvie piers. The deepest was 89 ft. below high water. Instead of a sloping surface of rock the bed of the Forth was of soft mud to a considerable depth, through which the caissons had to be sunk into the hard boulder clay. The process of sinking was as follows: The caisson being seated on the soft mud which, of course, practically filled the working chamber, air was blown in and a few men descended the shaft or tube of access to the working chamber in order to clear away the mud. This was done by diluting it to the necessary extent by water brought down a pipe under pressure, and by blowing it out in this liquid state through another pipe by means of the pressure of air in the chamber. It was found that the mud sealed the caisson, so that a pressure of air considerably in excess of that of the water outside could be kept up, and it was unnecessary to vary the pressure according to the height of the tide. In working through this soft mud both intelligence and courage were called for on the part of the men, and it is a pleasure and duty for me to say that the Italians and Belgians engaged on the work were never found wanting in those qualifications. There was always a chance of the caisson sinking suddenly or irregularly, and imprisoning some of the men, and indeed on one occasion a few men were buried up to their chins in the mud, and on another the caisson gave a sudden drop of 7 ft.

With one of our caissons we unfortunately had an accident and loss of life, which, although it had nothing to do with the sinking of the caisson, was indirectly due to the same cause, viz. the softness of the mud bottom. On new year's day, 1885, the S.W. Queensferry caisson, which had been towed into position, and weighted, with about 4000 tons of concrete, stuck in the mud, and instead of rising with the tide remained fixed so that the water flowing over the edge filled the interior. The 4000 tons of water caused the caisson to sink further in the mud, especially at the outer edge, and to slide forward and tilt. The contractors determined to raise the skin of the caisson until it came above water level, and then pump out and float the caisson back into position. About three months were occupied in doing this, but when pumping had proceeded a certain extent the caisson collapsed owing to the heavy external pressure of the water, and two men were killed.

Superstructure.

I must now say a few words respecting the design, manufacture, and erection of the superstructure.

Design.—I have already illustrated the principle of the cantilever bridge, and need only deal with the details. At the Forth, owing to the unprecedented span and the weight of the structure itself, the dead load is far in excess of any number of railway trains which could be brought upon it. Thus the weight of one of the 1700 feet spans is about 16,000 tons, and the heaviest rolling load would not be more than a couple of coal trains weighing say 800 tons together, or only 5 per cent. of the dead weight. It is hardly necessary therefore to say that the bridge will be as stiff as a rock under the passage of a train. Wind even is a more important element than train weight, as with the assumed pressure of 56 lbs. per square foot the estimated lateral pressure on each 1700 ft. span is 2000 tons, or two and a half times as much as the rolling load. To resist wind the structure is "straddle legged," that is, the lofty columns over the piers are 120 ft. apart at the base, and 33 ft. at the top. Similarly, the cantilever bottom members widen out at the piers. All of the main compression members are tubes, because that is the form which with the least weight gives the greatest strength. The tube of the cantilever is, at the piers, 12 ft. in diameter and $1\frac{1}{4}$ in. thick, and it is subject to an end pressure of 2282 tons from the dead load, 1022 tons from the trains, and 2920 tons from the wind; total, 6224 tons, which is the weight of one of the largest transatlantic steamers with all her cargo on board. The vertical tube is 343 ft. high, 12 ft. in diameter, and about $\frac{5}{8}$ in. thick, and is liable to a load of 3279 tons. The tension members are of lattice construction, and the heaviest-stressed one is subject to a pull of 3794 tons. All of the structure is thoroughly braced together by "wind bracing" of lattice girders, so that a hurricane or cyclone storm may blow in any direction up or down the Forth without affecting the stability of the bridge. Indeed, even if a hurricane were blowing up one side of the Forth and down the other, tending to rotate the cantilevers on the piers, the bridge has the strength to resist such a contingency. We have had wind gauges on Inch Garvie since the commencement of the works, and know, therefore, the character of the storms the bridge will encounter. The two heaviest gales were on December 12th, 1883, and January 26th, 1884. On the latter occasion much damage was done throughout the country. At Inch Garvie the small fixed gauge was reported to have registered 65 lbs. per square foot, but I found on inspection that the pointer could not travel further, or it might have indicated even higher. I did not believe this result, and attributed it to the joint action of the momentum of the instrument, and a high local pressure of wind too instantaneous in duration to take effect upon

a structure of any size or weight. The great board of 300 square feet area on the same occasion indicated only 35 lbs. per square foot, and I doubt much if the pressure would have averaged more than 20 lbs. on so large a surface as the bridge.

Manufacture.—The bent plates required for the tubes of the Forth Bridge would, if placed end to end, stretch 42 miles. Special plant had to be devised for preparing these plates. Long furnaces, heated in some instances by gas producers, and in others by coal, first heated the plates, which were then hauled between the dies of an 800 ton hydraulic press, and bent to the proper radius. When cool the edges were planed all round, and the plates built up into the form of a tube in the drilling yard. Here they were dealt with by eight great travelling machines, having ten traversing drills radiating to the centre of the tube, and drilling through as much as 4 in. of solid steel in places.

The tension members and lattice girders generally are of angle bars, sawn to length when cold, and of plates planed all round. Multiple drills tear through immense thickness of steel at an astonishing rate. The larger machines have ten drills, which, going as they do, day and night, at 180 revolutions per minute, perform work equivalent to boring an inch hole through 280 ft. thickness of solid steel every twenty-four hours.

Erection.—Facility of erection is one of the most important desiderata in the case of the Forth Bridge. Owing to 200 ft. depth of water, scaffolding is impossible, and the bridge has to constitute its own scaffolding. The principle of erection adopted was therefore to build first the portion of the superstructure over the main piers, the great steel towers, as they may be called, although really parts of the cantilever, and to add successive bays of the cantilever, right and left of these towers, and therefore balancing each other, until the whole is complete. This being the general principle a great deal yet remained to be done in settling the details. What was finally settled, and is now in progress, is as follows:—

After the skewbacks, horizontal tubes, and a certain length of the verticals as high as steam cranes could conveniently reach were built, a lifting stage was erected. This consisted of two platforms, one on either side of the bridge, and four hydraulic lifting rams, one in each 12 ft. tube. To carry these rams cross girders were fitted in the tubes capable of being raised so as to support the rams and platform as erection proceeded, and steel pins were slipped in to hold the cross girders. Travelling cranes are placed on the platforms, and these cranes, with the men working aloft, are of course raised with the platforms when hydraulic pressure is let into the rams. The mode of procedure is to raise the platform 1 ft. and slip in the steel pins to carry the load whilst the rams are getting ready to make another stroke of 1 ft. When a 16 ft. lift has been so made, which is a matter of a few hours, a pause of some two or three days occurs to allow the riveting to be completed. The advance at

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times has been at the rate of three lifts, or 48 ft. in height, in a week.

The system of erection by overhanging offers great advantages as regards safety, as each successive part of the superstructure is riveted up and completed before a further portion is added. In the case of an ordinary bridge the whole superstructure must first be temporarily bolted up on scaffolding, and in that condition is liable to be swept away by flood or hurricane at any moment.

There is nothing new under the sun, and therefore you will not be surprised to hear that in 1810, a certain Mr. Pope proposed to construct a cantilever bridge, of 1800 ft. span, across the East River, in New York, and, indeed, exhibited a 50 ft. model at the same time.

I have described the process of erecting the Forth Bridge in sober prose, if I had thought of doing it in verse I should have appropriated bodily Mr. Pope's lyrical version of his intended operations at the East River, of which the following is a sample :—

> "Each semi-arc is built from off the top,
> Without the aid of scaffold, pier, or prop;
> By skids and cranes each part is lowered down,
> And on the timber's end grain rests so sound.
> Sure all the bridges that were ever built,
> Reposed their weight on centre, pier, or stilt;
> Not so the bridge the author has to boast,
> His plan is sure to save such needless cost;
> A ladder on each side is lowered down,
> And shifted from the fulcrum to the crown."

To carry out the work at the Forth Bridge there is an army of 3500 workmen officered by a proportionate number of engineers. Everything except the rolling of the steel plates is done on the spot, and consequently there are literally hundreds of steam and hydraulic engines and other machines and appliances too numerous to mention, many of them being of an entirely original character.

It is, of course, impossible to carry out a gigantic work of the kind I have had the honour of bringing before the Institution without paying for it, not merely in money, but in men's lives. I shall have failed in my task if you do not, to some extent, realise the risks to which zealous and plucky workmen will be sure to expose themselves in pushing on with the work of erecting the Forth Bridge. Speaking on behalf of the engineers, I may say that we never ask a workman to do a thing which we are not prepared to do ourselves, but of course men will, on their own initiative, occasionally do rash things. Thus, not long ago a man trusted himself at a great height to the simple grasp of a rope, and his hand getting numbed with cold he unconsciously relaxed his hold and fell backwards a descent of 120 ft., happily into the water, from which he was fished out little the worse after sinking twice. Another man, going up in a hoist the other day, having that familiarity with danger which breeds contempt, did not trouble to close the rail, and stumbling backward, fell a

distance of 180 ft., carrying away a dozen rungs of a ladder with which he came in contact, as if they had been straws. These are instances of rashness, but the best men run risks from their fellow workmen. Thus a splendid fellow, active as a cat, who would run hand over hand along a rope at any height, was knocked over by a man dropping a wedge on him from above and killed by a fall of between one and two hundred feet. There are about 500 men at work at each main pier and something is always dropping from aloft. I saw a hole 1 in. in diameter made through the 4 in. timber of the staging by a spanner which fell about 300 ft. and took off a man's cap in its course. On another occasion a dropped spanner entered a man's waistcoat and came out at his ankle tearing open the whole of his clothes, but not injuring the man himself in any way.

Happily there is no lack of pluck amongst British workmen; if one man falls another steps into his place. Difficulties and accidents necessarily occur, but like a disciplined regiment in action we close up the ranks, push on, and step by step we intend to carry on the work to a victorious conclusion.

[B. B.]

WEEKLY EVENING MEETING,

Friday, May 27, 1887.

The Right Hon. Earl Percy, Manager and Vice-President, in the Chair.

Edward E. Klein, M.D. F.R.S.

Etiology of Scarlet Fever.

Among the infectious or zymotic diseases there are two at any rate — namely, scarlet fever and diphtheria — of which it may be said that their spread is to a lesser extent dependent on defective domestic sanitation than is the case with some of the other zymotic diseases, as, for instance, typhoid fever. Indeed, it is maintained by competent authorities that scarlet fever and diphtheria do not invade houses of faulty sanitation with greater frequency or severity than those of perfect sanitary arrangements. This view is based on the important experience gained during the past twenty years—viz. that epidemics of scarlet fever and diphtheria have been brought about by milk. I may here state by way of explanation that a fact well established and needing no further comment is that scarlet fever and diphtheria are, like smallpox, measles, whooping cough, and typhus fever, communicable directly from person to person. This mode of infection, doubtless an important one, and coming into operation in single cases wherever the elementary rules of isolation and disinfection are transgressed, altogether sinks into insignificance when compared with the infection produced on a large scale, if a common article of diet like milk should become in some way or another the vehicle of contagium, as has been proved to be the case in a number of epidemic outbreaks. These epidemics, known as milk scarlatina, milk diphtheria, and I may also add milk typhoid, have this in common, that almost simultaneously, or at any rate within a short time, in a number of houses having no direct communication by person or otherwise with one another, there occur, sometimes singly, sometimes in batches as it were, cases of illness—scarlet fever, diphtheria, or typhoid fever as the case may be; and it was this peculiar character which pointed to a condition which must have been common to all these households. On closer examination it was indeed found that all these households had this, and only this, in common, that they were all supplied with milk coming from the same source—that is to say, from the same dairyman. Other houses, supplied with milk from a different source, escaped; and, further, it was shown that, as soon as the consumption of the suspected milk ceased, the epidemic, as such, came to an end, except, of course, the cases due to secondary infection from person to person.

The Medical Department of the Local Government Board have

had for years past their attention fixed on these milk epidemics, and in the Reports of the Medical Officer many of these are described with great detail; amongst these Dr. Ballard's Report in 1870 on Enteric Fever in Islington, Dr. Buchanan's on Scarlet Fever in South Kensington in 1875, and Mr. Power's on Scarlet Fever in St. Giles's and St. Pancras in 1882, are especially to be referred to. Mr. Ernest Hart has tabulated all the outbreaks of milk epidemics which were investigated before 1881 in vol. iv. of the Transactions of the International Medical Congress for 1881. Now, analysing these outbreaks as far as they refer to scarlet fever, there are several of them where the assumption that the milk consumed acquired the power of infection by contamination from a human source cannot be excluded. This infection, if proven, would stand on the same footing as if due to contagion from person to person; for it is clear, whether the contagium is conveyed from one person to another by air, food, drink, or otherwise, it always remains contagion from person to person. Now, in many of the cases of epidemics tabulated by Mr. Hart, and recorded by subsequent observers—i. e. after 1881—this mode of milk contamination cannot be excluded, as I said before; but, comparing the dates when the milk was supposed to have become so contaminated with the dates when the milk has actually produced infection, it will be found that a certain discrepancy exists, and, as will be shown later, another mode of infection—viz. from a person affected with scarlatina to the cow and through the cow to the milk and then to human beings—cannot be excluded either. There are other epidemics recorded in these tables in which the mode of infection of the milk is not ascertained, and in a third set the milk acquired infective power in some way or another, but certainly not from a human source.

As an illustration of the first group of epidemics—i. e. probable contamination from a human source—I will refer to a case recorded by Dr. Robertson, of Keswick, in which a dairy closely adjoined a house where scarlet fever had existed for several weeks. The cows were milked, every night and morning, into an open tin can, which was carried across an open yard, past the affected house. The children who first caught scarlet fever in the locality played about the yard while in a state of desquamation. On one particular day a general epidemic of scarlet fever broke out in the town, between thirty and forty families being invaded. All those suffering from the disease received their milk supply from this particular dairy-farm. Some member of every family supplied had either a scarlatinal sore throat or scarlet fever on this day. Other families supplied from a different source escaped the disease. A lodger had the milk raw for supper and was attacked. His landlady boiled her milk the same night and escaped. We must here observe that a large number of fresh cases of scarlatina occurred on the same day, and the inference from this fact is that on some other day, shortly before, when the children who were peeling from recent scarlet fever were playing in the yard, they conveyed infection to the milk which was in their neighbourhood.

As an illustration of the second kind (viz. not probably from a human source) I will refer to the outbreak of scarlet fever in Oxford in the spring of 1882, recorded by Dr. Darbishire: in the St. Bartholomew's Hospital Reports, vol. xx. The substance of Dr. Darbishire's Report is this:—Three cows were kept by those who sold the milk, and nine houses, containing 85 persons in all, were supplied morning and evening; the milk was never stored, as there was generally barely enough at each milking for all the customers. In the house to which the cows and paddock belonged there was a case of diphtheria in a young lady; she was removed to the infirmary on March 1. The cowman had a child ill with scarlet fever in his cottage from February 27 till March 3. On March 3 Dr. Darbishire had this child removed to the hospital and the cowman's cottage thoroughly disinfected; the cowman left his cottage to sleep in lodgings near, the care of the cows having been handed over to another man, engaged for that purpose. Now, if the milk had become infected from either of these two cases (one diphtheria and the other scarlet fever) this must have occurred for the first before March 1, for the other before March 3; and as the period of incubation of scarlet fever is known to be as a rule less than seven days, it follows that March 3 being the last day on which the milk could have received the contagium from a human being, March 10 would be the last day on which scarlet fever could have been produced by that milk, the majority of cases of scarlet fever must have occurred before that day, as one cannot assume that in all these cases the period of incubation was protracted to such length as seven days. But mark what really did happen. Dr. Darbishire states that no case occurred till March 10, on which day two cases of sore throat and one case of scarlet fever occurred; on March 11, one case of sore throat; on March 12, two cases of sore throat and one of scarlet fever; on March 13, four cases of sore throat and two of scarlet fever; on March 15, one case of sore throat and one of scarlet fever; on March 16, two cases of sore throat and one of diphtheria; on March 17, one case of sore throat; on March 18, one case of sore throat. Now, all these cases were proved by Dr. Darbishire to have been caused by that milk. There occurred subsequently other cases, but these were traced to have been due to secondary infection from person to person. This is a good illustration of a milk epidemic in which the milk most probably was not fouled by human agency; and there are other milk epidemics which on analysis of dates lead to the same conclusion. The infection of this milk was probably brought about, as I shall show you hereafter, in some other way.

As an instance of the third kind—viz. where milk has clearly not been infected from a human source—I will refer to Mr. Power's Report in 1882 on an epidemic outbreak of scarlet fever in St. Giles's and St. Pancras. "The disease was distributed with a milk service derived from a Surrey farm. In this case two facts could be affirmed: the one that a cow recently come into milk at this farm

had been suffering from some ailment, seemingly from the time of her calving, of which loss of hair in patches was the most conspicuous manifestation; the other that there existed no discoverable means by which the milk which had coincided with scarlatina in its distribution could have received infective quality from the human subject." *

The Medical Department of the Local Government Board have from these facts drawn the conclusion that "distrust must be placed on the universally accepted explanation that milk receives infective properties directly by human agencies," and further that "the question of risk from specific fouling of milk by particular cows suffering, whether recognised or not, from specific disease was seen to be arising." This view received striking confirmation and proof by a report of an outbreak of scarlet fever that occurred at the end of 1885 and the beginning of 1886 in the north of London, which was investigated by Mr. Power. His report is published *in extenso* in the Report of the Medical Officer of the Local Government Board for 1886; and I will here give you the substance of it. Mr. Wynter Blyth, Medical Officer of Health for Marylebone, had last December observed a sudden outbreak of scarlatina in his district to be associated with the distribution of milk coming from a farm at Hendon, and had found reason for believing that the disease had prevailed exclusively among customers furnished with milk from that source. Mr. Power, on a more extended inquiry, found that a similar prevalence of scarlatina had occurred about the same time in other parishes in and near the metropolis that were furnished with milk from the same farm. By careful inquiry Mr. Power could with certainty exclude any contamination of the milk from a human source, or that anything of the kind known as "sanitary" conditions could have had any concern with the infectivity of the milk. Mr. Power showed conclusively that only certain sections of the milk supplies of this farm, and finally only certain cows from which these sections of milk were derived, had any relation to the observed results. "In the end," says the Medical Officer, "he has demonstrated, beyond reasonable doubt, the dependence of the milk-scarlatina of December on a diseased condition of certain milch cows at the farm, a condition first introduced there in the previous month by some animals newly arrived from Derbyshire; and he finds strong circumstantial evidence for believing that the latter phenomena of this dependence were brought about through the extension of the diseased condition of one set of animals to another set, after the fashion of an infection." Now, this disease as it presented itself in some of these Hendon cows consisted in the presence of sores and scurfiness in different parts of the skin with loss of hair in patches, ulcerations on the udder and teats, and a visceral disease, notably of the lungs, liver, kidney, and spleen, which, although milder in character, very much resembled the visceral lesions occurring in cases of human scarlet fever. By experiment it was shown that the

* Medical Officer's Report for 1885-86, pp. v. and vi.

matter of the ulcers of the udder is possessed of infective power, inasmuch as on inoculation into the skin of calves the same ulcers are reproduced; further it was shown that in the ulcers of the cow there existed in large numbers a species of micrococcus which on being planted on artificial nutritive media, such as are used for the study of bacteria, produces in a few days a crop of micrococci, possessed of very distinct characters by which they are distinguishable from other bacteria. When calves are inoculated from a cultivation of this micrococcus they become after an incubation period affected with a cutaneous and visceral disease the same as the disease of the Hendon cows. To sum up, then, it has been shown that at this Hendon farm there existed certain cows affected with a communicable disease which, in many points of its pathology, bears a great resemblance to human scarlatina; further, that the milk of these cows gave scarlet fever to human beings; and, lastly, that a particular microbe was obtained from these cows which in calves produced a similar disease to the disease of those cows.

In order to complete the evidence thus far obtained, it was necessary to prove that scarlet fever in man is due to the presence and multiplication in the blood and tissues of the same micrococcus, and that this microbe, if obtained from human scarlet fever, produces in the cow the same disease as is produced by the micrococcus of the Hendon cows. Now, this proof has been satisfactorily given. In the first place, it has been shown that in the blood and tissues of persons affected with scarlet fever there occurs the same micrococcus as was present in the cow, both being identical in microscopical and in cultural characters. In the second place it was found that the action of this microbe on animals is exactly the same as the micrococcus found in the Hendon cows. Calves and mice after inoculation or feeding with a trace of the growth of both sets of micrococci become affected with cutaneous and visceral disease similar to human scarlet fever; in calves the disease is of the same mild type as in the Hendon cows. Further it was shown that from the blood and the tissues of these animals infected with one or the other set of cultivations, the same micrococcus was recovered. I will remind you that in all infectious diseases which have been proved definitely to be associated with a particular species of microbes this microbe introduced into a susceptible body thrives and multiplies and thus sets up the diseased condition, differing, of course, with the different species of microbes. I think I may after this say that this microbe, *micrococcus scarlatinæ*, is the cause of human scarlet fever; further that it produces in bovine animals a disease identical with the Hendon disease and human scarlet fever, and that consequently while the cow is susceptible to infection with human scarlet fever, it can in its turn be the source of contagium for the human species, as was no doubt the case in that milk epidemic from the Hendon farm.

I shall now give you a striking piece of evidence well in harmony with what I have mentioned hitherto. In October 1886, Professor

Corfield forwarded to me certain tins of condensed milk, sold under the name of Rose brand. This milk was under suspicion of having produced scarlet fever in a number of persons who had partaken of it. From one out of three tins of this condensed milk I have obtained by cultivation a microbe which in every respect, morphologically and in cultures, is the same as the microbe obtained from the Hendon cows and from human scarlet fever. The action of the microbe of the condensed milk was also tested on animals, calves, and mice, and it was found that it produced the identical disease which was produced by the microbe of human scarlet fever and of the Hendon cows. I may add that this Rose brand of condensed milk is, like all condensed milk, obtained from cows' milk; the Rose brand is a cheap article, and meant for the poorer classes; probably it has not been sufficiently heated in the tins before sealing the latter; that this is probably the case can be deduced from the fact that every tin of this brand which I opened contained some organisms; thus, for instance, I find that one tin contained the scarlet fever microbe, and another species of micrococcus; another tin contained a harmless species of micrococcus only, and a third tin opened contained a micrococcus and a species of bacillus.*

Another piece of interesting evidence concerning the *micrococcus scarlatinæ* is this. There occurred during the beginning of this year a severe epidemic of scarlet fever in Wimbledon. This epidemic was also traced to milk coming from a particular farm. In one of the houses supplied with this milk there occurred cases of scarlet fever among human beings, and at the same time a pet monkey who also consumed a good deal of the milk became ill; it died after five days. I had the opportunity to make a *post mortem* examination of this animal, and there could be no doubt about its having died of scarlet fever. From the blood of this monkey I obtained by cultivation the same micrococcus as was obtained from human scarlet fever, from the Hendon cows, and from the condensed milk. Experiments made on animals with this micrococcus of the Wimbledon monkey showed that the same disease is produced both by inoculation and by feeding. It having been proved, then, that the cow is susceptible to infection with scarlet fever from man, the next important question is this. How does the milk of such infected cows assume infective power? Clearly in one of two ways—First, either the milk becomes infected by the milker during the process of milking, particles of contagium being rubbed off the ulcers of the udder or teat; or, the milk *per se* is possessed of infective power—that is, it being a secretion of a constitutionally diseased animal. From previous and from more recent observations I am inclined to think that both views hold good.

I now come to the question—How is the spread of scarlet fever by

* It is well known that no species of micrococci hitherto known are capable of surviving a temperature of 212° Fah., i. e. of boiling water; many of them are killed by an exposure to 180–190° Fah.

milk to be controlled and checked? This question resolves itself into three parts. First, prevention of infection of the cow by man, directly or indirectly; second, prevention of infection of the cow by the cow; and, third, destruction of the contagium of the milk of such cows.

As regards the first, all those rules which have been laid down to prevent infection of one human being from another, of milk or any dairy utensil by contact or otherwise with a person suffering from scarlet fever or coming from an infected house, also apply here; and this part of the subject comes under the general aspect of the proper sanitary management of dairies which is acted upon in all well-managed dairies.

As regards the second—viz. prevention of infection of the cow by the cow—this is obviously more important and more difficult of carrying out. I say obviously, because one cow affected with the disease is capable of communicating it to others in the same farm, and when moved to another farm also to the cows there. The disease in the cow being of a mild character is easily overlooked. The disease in the skin of the cow may be present and slight, or may be absent in its more conspicuous manifestation, whereas the visceral disease is of so mild a character that it requires an expert to diagnose it. When a cow shows the disease of the skin and on the udder well pronounced, such an animal will have to be carefully examined for visceral disease. I need hardly say that among the many cutaneous diseases of the cow known and unknown there may be one or the other which bears a resemblance to the cutaneous disorder occurring in scarlatina. Such cutaneous disease must be carefully excluded before an animal is condemned; but if visceral disease should be diagnosed as well, the animal should be carefully isolated and its milk should not be used. And it must be clear from this that every dairy should be permanently under the supervision of an expert, and in this the veterinary profession should be as eager for the work as the medical sanitary officers are and for some time past have been. But, judging from the attitude assumed by the veterinary authorities, I am afraid the veterinary profession has not yet grasped the full responsibility that rests on them, both towards the general public and the dairy farmers. Instances are on record when on the milk from a particular farm having been proved or even suspected to bear any relation to a scarlet fever epidemic, the business of such farm became temporarily or even permanently suspended, and the pecuniary loss of the owner of such farm irrevocable. That the disease in the cow which I have described to you as scarlet fever is as yet unknown to the veterinary profession does not do away with the existence of such disease, and I venture to say that being as yet unknown to and unrecognised by them should be so much more stimulus for their trying to recognise it.

Now the third question, as to the destruction of the contagium in the milk. This, I am glad to say, is very easily carried out. I have

found that heating milk up to 85° C., or 185° Fah.—that is, considerably under the boiling point, is perfectly sufficient to completely destroy the vitality of the microbe of scarlet fever. In support of this statement I can quote, besides the observation given above by Dr. Robertson, the following observations recorded by Dr. Jacob, Medical Officer of Health of High Ashurst and Headley, and reported in 1878, to this effect:—Between June 1 and 7 there were fifteen cases of scarlet fever in three distant houses, the inmates of which had had no communication with infected persons, but had all been supplied with milk from a farm where a certain cowman worked. This cowman had in his family several children ill with scarlet fever. The cowman continued milking the cows during the illness of his children, though he did not himself have the fever and the milk was not taken into his cottage, but the point which I wish to bring out is this, that other houses besides those in which scarlet fever had broken out had been supplied with the same milk, but no scarlet fever occurred in them, and why? Because all these have consumed only the scalded milk. I should therefore strongly urge that all milk should have been boiled, at any rate heated to at least 85° C. (that is, 185° Fah.) before being consumed. And, judging by the large number of cases of scarlet fever recorded in these milk epidemics, one is justified in saying that a considerable percentage of the total number of cases of scarlet fever would have been avoided thereby. Not all, because unfortunately the rules of isolation of patients suffering from scarlet fever are not always rigorously carried out, and therefore infection from person to person will occur. Nor would prevention of scarlatina by milk exclude scarlatina by cream. Cream cannot be subjected to heat, and in the epidemic of scarlet fever that occurred in South Kensington in 1875, and that was investigated by Dr. Buchanan, cream was the vehicle of contagium. But, considering the prominent position that milk occupies in every household with children, the possibility of infection with scarlet fever by raw milk deserves careful attention.

The lecture was illustrated by demonstrations of the *micrococcus scarlatinæ*, in microscopic specimens and in culture tubes obtained from the several sources indicated.

WEEKLY EVENING MEETING,

Friday, June 3, 1887.

Edward Woods, Esq. Pres. Inst. C.E. Manager and Vice-President, in the Chair.

David Gill, LL.D. F.R.S.

HER MAJESTY'S ASTRONOMER AT THE CAPE OF GOOD HOPE.

The Applications of Photography in Astronomy.

Little more than a year ago Mr. Ainslie Common delivered a lecture in this place on the subject of "Photography as an aid to Astronomy." Given by one who is a consummate master of the art of celestial photography, that lecture (complete as to history and full of suggestion as it was) would, under ordinary circumstances, have precluded further reference to the subject in these Friday evening lectures for some years to come.

But the past year has witnessed such developments of the subject, and the importance of photography in astronomy has been so much advanced by the conclusions of the recent Astro-photographic Congress, as to afford a reasonable apology for the present lecture.

On the 16th of April last there was held at Paris a Congress attended by upwards of fifty astronomers and physicists, representing nearly every civilised nation in the world. It was convened for the purpose of considering a scheme of international co-operation in the work of charting the sky on a large scale. Or, rather, its object was to obtain a series of pictures, which, taken within a comparatively limited period of time, and with the necessary precautions, would enable astronomers of the present day to hand down to future generations a complete record of the positions and magnitudes of all the stars in the heavens to a given order of magnitude. The labours of that Conference are now concluded, certain important resolutions have been adopted, and the way has been so far cleared for giving these resolutions practical effect.

It seems of importance therefore to lay before the members of the Royal Institution some account of the history of this remarkable Congress, to illustrate and explain the grounds of the conclusions which it has arrived at, and otherwise to bring the history of photographic astronomy up to the present date. I pass over the already well told early history of celestial photography, except in so far as it relates to star charting. It was Warren de la Rue who first called attention to the means furnished by photography for charting groups of stars. In his Report to the British Association at Manchester in 1861 on the progress of celestial photography, he indicates a photographic

object-glass of short focus as the instrument best suited for the purpose, and he states that, by mounting such a lens and camera on an equatorial stand provided with clockwork, he has photographed such groups of stars as the Pleiades, the chief difficulty being not to fix the images of the stars, but to distinguish them from the specks which are found on the plates or rather in the collodion.

In 1864 Rutherford of New York completed a telescope of $11\frac{1}{2}$ inches aperture and 14 feet focal length, specially constructed for celestial photography, and obtained fine photographs of stars to the 9th order of magnitude. His remarks, although quoted in Mr. Common's lecture last year, have such importance on the present subject that I venture to repeat them.

"The power to obain imagest of the 9th magnitude stars with so moderate an aperture promises to develop and increase the application of photography to the mapping of the sidereal heavens and in some manner to realise the hopes which have so long been deferred and disappointed.

"It would not be difficult to arrange a camera-box, capable of exposing a surface sufficient to obtain a map of two degrees square, and with instruments of large aperture we may hope to reach much smaller stars than I have yet taken. There is also every probability that the chemistry of photography will be very much improved and more sensitive methods devised."

Mr. Common well remarks that in the light of recent work these words are almost prophetic.

But Rutherford did not stop here. In the eyes of an astronomer a picture of stars is of comparatively little importance unless it is capable of accurate measurement. Recognising this important feature of the case, Rutherford devised a suitable apparatus, which he applied to the measurement of two of his photographs of the Pleiades. These measures having been put into the hands of Dr. Gould, that astronomer compared them with those of the same group of stars made by Bessel with his celebrated heliometer, and found a satisfactory accordance.*

Encouraged by these results, Dr. Gould, when he went to the Argentine Republic in 1870 to found the Cordoba Observatory, which has since been rendered so famous by his labours, took Rutherford's telescope with him. Unfortunately one of the lenses was broken in transport, and such delay was incurred in replacing it, that the proposed work could not be begun till 1875. But thanks to the clear skies of Cordoba, and the marvellous activity of the observatory under Dr. Gould's direction, 1350 photographs were obtained in course of a few years, containing representations of all the principal star-clusters of the southern hemisphere, besides a special series of plates taken for the purpose of determining the parallax (or distance) of several of the more remarkable stars in the southern hemisphere.

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* Astron. Nach. No. 162, vol. xlviii. Dec. 1866.

This fine series of pictures is now being submitted to measurement by Dr. Gould, and the results are awaited with the greatest interest by all astronomers.

The first of Dr. Gould's plates were taken with the old wet collodion process, but the work was afterwards greatly facilitated by employment of the more sensitive modern dry plates.

It was, in fact, the introduction of the gelatine dry plate process in 1876, which really paved the way for the rapid development of celestial photography. The convenience of the manipulation and the great increase of sensitiveness of the plates at once placed a new power in the hands of astronomers. Draper photographed the nebula of Orion in 1880; and after trials, commencing in 1879, Common succeeded in obtaining the exquisite photographs of that object which have been exhibited more than once in this theatre.

In 1882 appeared the splendid comet of that year. At the Royal Observatory, Cape of Good Hope, we were not at the time engaged in photographic operations. Several photographers in the Cape Colony found it possible to obtain impressions of the comet, but they were unable to secure pictures of scientific value, because they were unprovided with means to follow the diurnal motion. I had no available camera belonging to the observatory, and no experience in the development of modern dry plates. In these circumstances, I applied to Mr. Allis, a skilful photographer in my neighbourhood, who eagerly consented to co-operate with me in the work. I arranged means to attach his camera to the stand of an equatorial telescope, and the telescope itself was employed to follow the nucleus of the comet accurately during the whole time of exposure by the aid of the driving clock and with small corrections given by hand. The lens employed had an aperture of only 2 inches, and a focal length of 11 inches; but the result was a series of pictures, one of which, obtained after an exposure of two hours, is now on the screen.

The photograph shows a very satisfactory delineation of the tail and envelope of the comet.

Important and useful as these results were, there was another feature of the pictures which seemed to me still more so. In forwarding copies of these photographs to the Royal Astronomical Society of London and to the Paris Academy of Sciences, I drew particular attention to the large number of stars shown upon the plate, and insisted upon the importance of the means thus offered to photograph comparatively large areas of the sky and thus rapidly make charts of the entire heavens.

The one step wanting was now provided, and the new and more sensitive dry plate rendered the former suggestions of de la Rue and Rutherford now valuable and practicable.

Formerly the old collodion wet plates required large instruments (with small field) and long exposure to depict stars even to the 9th magnitude, and astronomers trusted entirely to the accuracy of their

driving clocks, which could not follow a star with perfect accuracy during a long exposure. Now the modern rapid dry plates in conjunction with the large fields of the photographic objective overcame the first of these difficulties, and the plan of employing a guiding telescope overcame the second.

The use of a guiding telescope was not even a new device, for it had been employed long before by Hartnup and others, who, in their early attempts to photograph the moon, kept the image of a lunar spot by hand upon the cross wires of the finder of the telescope during the long exposures then necessary.

There was thus nothing really new either in my suggestion or in the *modus operandi*, only the result was a fortunate one, for Mr. Common says that "these photographs came to him as a revelation of the power of photography for the purpose of star-charting," * and Admiral Mouchez tells me that these Cape photographs and my suggestions first directed his attention and that of the brothers Henry to the application of photography to the work of star-charting, which had for many years been carried on at Paris by the older methods of astronomy.

Common was amongst the first to take up the work in England, and here on the screen is one of his photographs with a 4-inch lens, executed in December 1883. But being engaged in other researches, Common made no attempt to commence a systematic survey of the heavens.

Isaac Roberts, of Liverpool, was also early at work in the same field, and after preliminary experiments he acquired a powerful telescope, with which he began a systematic survey of the northern heavens.

It required some time to find the necessary means and apparatus to begin the realisation of my ideas at the Cape, but at last the work was started in the beginning of 1885 on the following definite plan, viz. to complete the cartography of the heavens from 20° south of the Equator to the South Pole, and so as certainly to include all stars to the 9th magnitude.

The reasons for the adoption of this plan were the following:—

The celebrated astronomer Argelander charted the heavens on this scale from the North Pole to the Equator, and the work has recently been extended to 20° south of the Equator by Schönfeld, the pupil and successor of Argelander.

Argelander's *Durchmusterung*, as it is called, has furnished, ever since the date of its publication, the nomenclature of all the fainter stars employed in the daily operations of astronomy; it has furnished the working catalogues which are essential for the more exact determination of the places of all these stars; it has given us the first accurate data for determining the distribution of the stars according to magnitude and apparent position in the heavens, and is the first

* 'Proc. Royal Institution,' vol. xii. part iii. p. 734.

solid existing basis for founding any theory as to the constitution of the stellar universe. To complete the Durchmusterung for the remaining portion of the heavens was therefore the most pressing need of modern astronomy. I commenced the work in 1885 by the aid of photography. I hope in two or three years, if I have the honour of lecturing again in this theatre, I shall then be able to tell you that the work in question is finished.

I should here explain that mere pictures of the stars are of comparatively little value, or rather of about the same value to an astronomer as a series of charts of parts of the world would be to a sailor if there were no lines of latitude or longitude marked upon them.

The every-day useful part of the Durchmusterung is the catalogue giving the positions and magnitude of all the stars. That work is rapidly advancing in the hands of my able and enthusiastic friend, Professor Kapteyn of Groningen, who, with the aid of three assistants, has undertaken to devote five or six years of his life to the measurement of the Cape photographs and the computation of the results.

When this has been done, as I venture to think it will be within five years, astronomers will be in possession of that preliminary survey of the whole heavens which is necessary for the more refined and elaborate researches which must follow as results of the Paris Congress.

But to return to the work that was meanwhile being done in Paris by the brothers Paul and Prosper Henry.

These astronomers had been engaged since 1871 in the construction of charts of the Ecliptic by the older processes of observation, but when they reached that portion of the heavens where the Milky Way crosses the Ecliptic, the number of stars became so overwhelming that the task of charting seemed almost too great for human patience and skill. But fortunately the time had come when dry plate photography could be called in to aid, and this aid was in the hands of men singularly competent to develop such an opportunity to the fullest extent. The brothers Henry had long aspired to be not only distinguished practical astronomers, but, following the traditions of Huyghens and the Herschels, they desired also to be the artists of their own optical means. Bound together by strong brotherly affection and common tastes, gifted alike with practical talents of a high order, and with an energy and determination of character that permit no obstacle to success, these men thus happily united have devoted the spare hours of their busy astronomical duties at the Paris Observatory, first to the study of optics, and afterwards to the grinding and polishing of lenses and specula, which have won for them a now world-wide reputation as opticians of the highest rank.

I had the pleasure, a few weeks ago, of visiting the modest workshop attached to their house at Montrouge, and I shall not soon forget that visit, nor the many lessons moral as well as practical which I learned.

Every detail of their process of working has been evolved by themselves; they employ no assistant, and their every appliance is simple and practical in a degree which I can only compare with the simple and practical character of the men who designed it.

Such were the men above all others to develop the application of photography to the charting of the heavens. They had high appreciation of the value of the work which they were about to undertake, they had the fullest knowledge of the requirements of the case, and they had the practical skill which enabled them to perfect the necessary apparatus. Their first attempts were made with a telescope of six inches aperture (the object-glass being specially ground for photographic work), and the tube was temporarily adapted to an existing equatorial stand.

With an exposure of forty-five minutes, pictures of stars were obtained to the 12th magnitude, in which the star discs were quite round and sharply defined.

Fully appreciating the beauty of this result, and seeing its importance, Admiral Mouchez boldly faced many administrative difficulties, and accepted without delay the proposals of the brothers Henry to construct an object-glass of thirteen inches aperture and about eleven feet focal length, as well as the offer of M. Gautier to mount the same on a suitable stand. The new instrument was mounted in May 1885. A photograph of the complete instrument is now on the screen.

Both from an optical as well as a mechanical point of view, the new instrument was admirably adapted for its intended work, and the results obtained by the brothers Henry, and rapidly published and circulated by Admiral Mouchez, at once astonished and delighted the astronomical world.

I now show a few of the more remarkable of these star pictures on the screen.

After such results as these there was no longer room for doubt or delay. The exquisite precision of these pictures, the sharpness and roundness of the images of the stars, and the results of actual measurement on the plates, proved that all necessary accuracy had been attained.

The means of rapidly obtaining the data for an accurate survey of the heavens on a very large scale were now within the reach of astronomers, and the time for decisive action had arrived.

The work, however, was too extensive to be undertaken at a single observatory, or even by a single country, and it was agreed on all hands that international co-operation was essential for its execution in a sufficiently short space of time.

I need not enter into the details of preliminary consultation or correspondence, but at last a time was fixed, and invitations were issued by Admiral Mouchez, Director of the Paris Observatory, under the auspices of the Paris Academy of Sciences, for an International Congress of Astronomers to be held at Paris.

A preliminary committee having arranged the general order of business, the Congress was opened on the 16th April, and its thoroughly representative character will be understood from the following statement of the nationalities of the members present.

France	20	Austria	2	Spain	1
England and Colonies	8	Sweden	2	Switzerland	1
Germany	6	Denmark	2	Portugal	1
Russia	3	Belgium	1	Brazil	1
Holland	3	Italy	1	Argentine Republic	1
U.S. America ..	3				

Before the Conference, a great many people, I will not say astronomers, held that the chief object was to photograph as many stars as possible, and simply preserve these plates or issue photographic copies of them, so that astronomers of the future, by merely comparing one of these originals or copies with a similar photograph of the same part of the sky taken 50 or 100 years hence, would find out what stars had changed in position or magnitude, or whether any new star had appeared.

There is no doubt this was the view of the popular writers—it is very easily understood, and it appeals very directly to the imagination. Such a project alone would no doubt have had great importance and would probably in the future have brought to light a great many very interesting isolated facts.

But for the broader and more refined purposes of astronomy, for the discussion of such great questions as the motion of the solar system in space, the common movement of large groups of stars, the accurate determination of precession, and the general refinement of astronomy of precision, these mere pictures would have no value.

It was essential for these larger and more permanently important ends that all data should be provided for the most refined determination of the *absolute* position of any star upon any plate. This view was endorsed by the Congress.

The objects of the survey of the heavens to be carried out were defined ultimately thus:—"To make a photographic chart of the sky for the present epoch, and to obtain the data for determining the positions and magnitudes of all the stars to the 14th magnitude," as that magnitude is at present defined in France.

At present there are no exact determinations of stellar magnitude to that order of faintness, and the considerations which really guided the Conference were, that stars which are called 14th magnitude are photographed by the Henrys with an exposure of about 15 minutes of time. With such an exposure the time required for the work contemplated by the Congress would not be too great, but to demand long exposures would lead to the loss of many plates by interruptions from clouds, &c., and would unduly prolong the time required for completion of the whole work. As it is, the number of stars photographed to 14th magnitude will number about 20 millions.

It was seriously urged that stars to the 15th or even 16th

magnitude and higher should be photographed, but it was felt that there was real danger of failure in an attempt to do too much.

It no doubt produces a strong effect on the imagination to be told that astronomers are to be engaged on making charts of the sky which will contain 60 or 100 millions of stars, or photographing stars on their plates which cannot be seen at all in the most powerful telescopes. There is thus a strong temptation to yield to this demand for sensation, to produce a few astonishing plates with the loss of much precious time, and to sacrifice the real progress of astronomy to the love of the marvellous. Besides, what are you to do with pictures of 100 millions of stars when you have got them? What would be the use of pictures of all these stars, unless at some future time a sufficient number of astronomers were to arise to compare similar photographs, taken, say one hundred years hence, with the photographs taken in our day? I am happy to think that the number of men who devote themselves to the pursuit of astronomy is on the increase, but I have no desire that the number of men in Great Britain who occupy themselves exclusively with astronomy will ever correspond with that in the floating island of Laputa, as described by Dean Swift, where all the men were exclusively occupied with astronomy, and had to be flapped on the head with little bladders containing parched peas to arouse them from their abstract occupations. And yet, unless something of this sort happens, I see no adequate prospect of the utilisation of pictures of 100 millions of stars.

The Congress, therefore, very wisely limited their chart plates to the 14th magnitude. But, as was well said by M. Bouquet de la Grye, it was not necessary to summon fifty or sixty astronomers to a Congress to arrange for taking mere photographs of stars—a number of photographers provided with instruments like the Henrys could have done all that without a congress. It was very strongly felt that the true *raison d'être* of the Conference was to secure astronomical data, precise and exact as the operations of astronomers should be.

Accordingly they resolved that—

"In addition to the duplicate series of plates giving all the stars to the 14th magnitude, there should be a series of plates of shorter exposure to insure a greater accuracy in the micrometric measurement of the standard stars, and to render the construction of a catalogue possible. The plates intended for the formation of the catalogue shall contain all the stars to the 11th magnitude inclusive." That is to say, it was determined to catalogue the absolute places of stars to the 11th magnitude.

But no photographic plate of itself gives us any information about the absolute places of stars, though it gives the means to determine the relative positions of the stars on the limited area of each plate; you must trust to the old-fashioned meridian observations to determine the absolute places of the brighter stars on each plate, and then measure the position of the fainter stars relative to these standard stars.

Now if a plate is exposed long enough to get satisfactory pictures of stars to the 14th magnitude, the images of the standard stars of the 7th, 8th and 9th magnitude will not have the highest perfection, and consequently the places of the fainter stars cannot be measured relative to the ill-defined standard stars with the highest precision.

This will be evident if we examine actual photographs.

One illustrates a short exposure, the other a long exposure. The short exposure gives sharp definition of the brighter stars, the long exposure brings into view a much greater number of stars, but the sharp definition of the brighter stars is completely lost. Therefore, if we wish to have determinations of *absolute* positions, we cannot have long exposures.

The meaning of the series of plates of short exposure, and showing stars only to the 11th magnitude, is thus explained:

Of stars to 11th magnitude there are about $1\frac{1}{2}$ millions in the sky, and a catalogue containing all these stars may be considered complete for the practical purposes of astronomy, because that magnitude is the faintest which can be measured with accuracy in the larger class of equatorials usually employed in working observatories.

I need not enter into detail about the technical means which are to be taken for eliminating the various sources of error, such as contraction of the photographic film in course of development, and so forth. All these points have been considered by the Congress, or put into the hands of specialists when it appeared that any particular point required further special study, and they are too technical to be entered upon here. The chart of stars to the 14th magnitude will be of importance for many purposes, such as the search for minor planets, and the trans-Neptunian planet, for variable stars, and for data as to the law of distribution of stars of the higher order of magnitude. But I do not hesitate to say that the work which astronomers of future generations will be most grateful for, and which will most powerfully conduce to the progress of astronomy, will *not* be the chart but the catalogue.

And now, Ladies and Gentlemen, I have dragged you through what I fear has so far been a weary account, to bring you to an apparently very uninteresting conclusion.

Catalogues and figures are not matters of much popular interest, and yet from such uninviting material has been built up the fair structure of the exact astronomy of the present day; and out of such materials have been evolved the facts which appeal so strongly to the minds of men, and most strongly so because men know that the conclusions rest not on mere imaginings alone, but on solid facts and figures also.

But now as to the practical execution of this useful work. After all the preliminary details of the operations have been fully discussed—when the instruments have been designed and made, and the mode of working and the methods of measurement and reduction have been devised, the practical execution of the work becomes one

long round of routine labour, requiring skilled and careful superintendence it is true, but still routine work of a very trying character.

Such work never has been, and never will be, the occupation of the amateur or single-handed astronomer. Essential as such work is to the progress of astronomy, it can only be executed at regular Government establishments, and therefore the conclusions of the Conference will have to be submitted to the various Governments, and the necessary votes of money must be secured. France has already definitely sanctioned the funds for four photographic telescopes of the kind which the Conference has decided to adopt for the work. And we cannot doubt that the modest claims which will be made on England's treasury for her share in this great work will be liberally responded to.

But there are other applications of photography to astronomy which have a daily growing importance. It was desirable that the Conference should recognise this work, and establish relations with those engaged upon it.

Accordingly the following resolution was passed:—

"The Congress expresses the desirability of there being a special committee which shall occupy itself with the applications of photography to astronomy, other than the construction of the chart. It recognises the importance of these applications and the relations which it is desirable to establish between different kinds of work. The Congress request Messrs. Common and Janssen to undertake the realisation of this proposition."

At first sight this may appear a somewhat barren resolution—but indeed it is not so. It must be remembered that the Congress was convened for the purpose of discussing a special object, it had arrived at definite conclusions and recommendations in connection with that object, and it was felt that to go beyond that object might imperil the adoption of its recommendations by the various Governments.

But in the hands of men like Common and Janssen the resolution of the Conference is not likely to be a barren one, indeed it is certain that it will not be so, for they are already taking steps to unite fellow-workers in this field.

Their Committee will associate itself with those who are engaged upon the Charts, and will follow up in detail and with special instruments and methods the subjects of interest which from time to time will be encountered by the routine workers.

So remarkable has been the progress of the miscellaneous application of photography to astronomy within the past year, that some account of it is essential to bring the history of the subject up to date.

For example, we have the recent work of Professor Pritchard, of Oxford.

He has applied photography during the past year to the most refined and difficult problem of practical astronomy, viz. the determination of the annual parallax (i. e. the distance) of the fixed stars.

He has selected for experiment the interesting double star 61 Cygni. One of the original negatives of the series is now on the screen. This star, as is well known, was selected by Bessel, on account of its large proper motion, as the most suitable star for his first experiment. It was probable, because its large apparent motion among the stars was so great, its real distance from us would be less than that of stars of less apparent motion. Bessel's observations with the Königsberg heliometer proved this to be the case, and his discussion of these observations first convinced astronomers that the measurement of interstellar spaces was a problem not entirely beyond their reach.

Prof. Pritchard has now photographed the star during a whole year, and within a few days he promises that we shall have the results of his measurement of the plates. It will be of great interest to compare his results with previous independent determinations of the parallax of the same star made by other astronomers with different means, but it will be still more interesting for the future of astronomy to compare the amount of accuracy which the photographic method affords, as compared with the older existing methods. From preliminary results published by Prof. Pritchard we are led to expect a very high accuracy from the new process.

So far, however, as present experience goes, we shall not be able to apply this new method to the measurement of the parallax of very bright stars, because, when the plates have been exposed long enough to obtain pictures of the faint comparison stars, the discs of the brighter stars become too large and ill-defined for exact measurement. It may be that this obstacle will yet be overcome, but at present it has still to be faced.

On the question of the comparative merits of refractors and reflectors as the proper instruments for photographic use, very elaborate comparison has been instituted, and much discussion has been held.

From the simple facts, that the best work yet done has been done in stellar photography by refractors, and that they are in many ways more convenient and simple in use than reflecting telescopes, the Paris Congress unanimously adopted the refractor as the instrument to be adopted for the international star charts. But here is a very remarkable picture taken with the Oxford reflector, which shows star discs very sharp and very round over a very large field of view, viz. eighty minutes of radius.

In the photography of special objects, such as star clusters and nebulæ, much has been done.

Common's exquisite photograph of the great nebula of Orion you have seen before in this theatre, and for exquisite beauty of detail it has never been excelled. But of this we may be sure that, if Mr. Common is spared in health and strength to complete the great reflector of five feet aperture upon which he is now engaged, that photograph, beautiful as it is, will be far surpassed. Here is another photograph of the same object by Mr. Roberts. So short is

the focus of his telescope, so sensitive are the plates he has employed, that the detail of the brighter parts has been completely burnt out, but a great deal of new found detail is brought to light.

Here is another photograph of the same object by Professor Pickering, taken with a four lens objective of eight inches aperture and very short focus, and including a field of 5 degrees square. Exposure 82 min. This shows what can be done with such a combination.

In 1885 the brothers Henry, photographing the Pleiades on November 16th, discovered a new nebula, near the bright star Maia in the group. Here on the screen is a copy of the original negative by which the discovery was made. You observe the nebula like a filmy projection from one of the stars.

After the nebula had been discovered by photography it was found to be visible in the great telescope of 30 inches aperture at Pulkowa. But to *discover* is one thing, to see after discovery is another.

Strangely enough this new nebula was really photographed a fortnight before its discovery at Paris, by Professor Pickering at Cambridge in America. In exhibiting the photograph to the National Academy of Sciences five days before Henry's discovery, Professor Pickering pointed out the "wing" attached to the star, but there was only one plate shown, the impression was that the mark was due to a defect in the gelatine film.

Here, however, is another picture of the Pleiades taken at Cambridge with the same instrument and an exposure of eighty-two minutes, which shows nebulosity about more than one star of the group.

And here is a copy of a negative by Mr. Roberts, of Liverpool, with an exposure of three hours. The star discs are of course large and ill defined; but the quantity of nebula, invisible to the eye in the largest telescopes, is quite surprising.

These photographs appear to prove conclusively that the nebula and the stars in this group are one system; the doctrine of chances renders it almost an impossibility to suppose that such a symmetrical arrangement of nebulous matter with respect to the stars could exist by chance, if the stars were projected in front of a far distant background of nebulous matter.

Here is a photograph of the stars surrounding the celebrated variable star η Argus, taken at the Cape with the telescope of 9 inches aperture, generously presented to me for such work by Mr. James Nasmyth. The nebula surrounding this star is very faint compared with the Orion nebula, and it seems to be deficient in actinic rays, and besides, the telescope is intended for stellar photography by its long focal length, and not for nebulæ, which require a shorter proportional focus—i. e. more intrinsically brilliant image.

Still there is the nebula, and I believe this is the only existing photograph of the object. The exposure was $2\frac{3}{4}$ hours, and yet although the original negative has been enlarged four diameters the

star discs remain well defined. The corresponding region of the sky is less than the moon's apparent diameter, and of the many thousands of stars visible on the photograph not a single one is visible to the naked eye. The star η Argus was in 1843 nearly the brightest star in the heavens; in fact, second only to Sirius. It is now between the 7th and 8th magnitude.

Here is a star-cluster in Argus. The star discs are not so sharply defined, but the original negative has been much magnified to bring out the star discs.

Here is a photograph, also taken at the Cape, of the wonderful star cluster ω Centauri. It is the finest globular cluster in the heavens, and I do not know that I have ever seen the separation of the central stars so distinctly with the eye as they are shown in this photograph. Perhaps by photographing we shall learn what motions occur in each cluster. This negative has been enlarged four diameters from the original.

Here is a photograph of the well-known cluster in Hercules, taken by Mr. Roberts, of Liverpool, and a still more wonderful one by the Henrys, of Paris. They must be magnified more highly to give any idea of their quality.

When the objects are bright, such as bright double stars, or planets, or the moon, we can enlarge the image produced by the telescope, by aid of a secondary magnifier.

Because of the greater size of the original pictures thus produced, the granulation of the photographic film interferes to a less extent with the detail of the picture. Of course, this advantage is purchased at the cost of a longer exposure, because the same amount of light is spread over a larger area of the sensitive plate, and consequently the same area of the film receives less intense light. With very bright objects, such as the sun, moon, and planets, this is of little consequence, and may be an advantage, as permitting more accurate regulation of the exposure.

Here is a picture of the sun photographed by M. Janssen at Meudon, near Paris. The exposure is less than 1/1000th of a second of time. And here is an enlarged photograph of the same spot, showing an amount of detail which no artist could convey by hand and eye, nor could he emulate the absolute accuracy of the photograph.

Here are some photographs of the planet Jupiter, taken at Paris, the original image being magnified 18 times.

Here is another showing the remarkable red spot—you even have before your eyes evidence of the rotation of Jupiter on its axis by the change in the position of the spot during the same evening.

This spot appeared in 1878 and measured about 30,000 miles in length by 7000 miles in breadth. It became of a deep red colour in 1879, and for the three following years was a most striking feature in the planet. It almost faded entirely in 1883, but has again become nearly as bright as it was in 1882.

Miss Clerke tells the story most admirably and suggestively in the last edition of her 'History of Astronomy,' to which work I would refer those of my hearers in whom these beautiful photographs may excite a sufficient interest.

To enter fully into the matter would demand a lecture to itself—and the minute hand of that inexorable clock warns me that I must move on.

Here are some photographs of Saturn, which illustrate the remarkable progress of celestial photography.

Here are some photographs of double stars : one of these, a photograph of γ Virginis, taken at Greenwich, is probably the finest photograph of a double star in existence. The star discs measure less than 1″ in diameter.

Last of all I come to the most recent revelations of the power of photography as an aid to astronomy. Dr. Henry Draper, in 1872, was the first to photograph the lines in the spectrum of a star, but his admirable investigations were interrupted by death in 1882. In 1886, his widow placed in the hands of Professor Pickering, of Harvard College Observatory, in America, not only an ample sum of money for the purchase of costly apparatus, but also made a liberal provision for carrying on the work of photographic spectroscopy as a memorial to her husband. So noble a gift, and the execution of so pious a purpose, could not have been placed in abler or more active hands.

Within the past few weeks we have received the first-fruits of the Henry Draper Memorial Fund.

When I began preparation of this lecture, I cabled to Professor Pickering a request for some glass copies of his original negatives. He kindly complied, and they arrived this morning. Time only permits me to show them rapidly, but those who remember Dr. Huggins's lectures on stellar spectra in this theatre, will recognise the enormous importance of such pictures as these.

The ingenuity of the adopted methods, the extraordinary success attained, the promise of rich harvest, exceeding our highest previous expectations, which the results afford, are themes upon which one could dilate for hours.

Here we have the spectra of the distant stars whose actual discs we can never hope to see, registering in these rhythmical lines the story of their constitution and temperature, with an accuracy and precision which not many years ago we should have been glad to obtain in the records of the spectrum of our own sun.

And this is not all ; not only have we such results for a few stars, but we are promised " that the complete work will include a catalogue of the spectra of all the stars of the 6th magnitude and brighter, a more extensive catalogue of spectra of stars brighter than the 8th magnitude, and a detailed study of the spectra of the bright stars." These are Prof. Pickering's own words.

What Prof. Pickering promises, we know from long experience,

that he will perform. We may also well say with him, that "a field of work and promise is open, and there seems to be an opportunity to erect to the name of Dr. Henry Draper a memorial such as heretofore no astronomer has received."

There is in England wealth enough and to spare. Many a rich man dies puzzled how to dispose of his money ; and there is many a living man who would gladly give for such an object if he knew how to do so. There is field enough in astronomy, and there are men enough in England to do the work. Let us hope they will receive aid such as Prof. Pickering has received; and having done so, they will give an equally good account of their stewardship.

The miscellaneous applications of photography to astronomy offer a field so full of promise, so certain of immediate reward to those who are possessed of the necessary originality and the means to carry out their ideas, that there is more hope of private enterprise in that direction than in the more routine work of star-charting.

But tempting as these fields are, brilliant and interesting as are the discoveries to be found in them, there is in the work instituted by the Paris Congress an element that cannot be overlooked and which compels attention—it is this: the question of the lapse of time. Every year which passes after that work has been carried out, increases its value and importance; every year that we neglect in doing it will be a reproach to the astronomers of the day. Into all the great problems which that work is destined to solve, the element of time enters—and time lost now in such work can never be recalled.

Of the Congress itself I would say a few last words. Its proceedings were characterised by an earnest spirit of work and entire absence of international jealousy. Our reception by the French was cordial and hospitable in the highest degree, the decisions of the Congress were almost unanimous, and were marked by a moderation and judgment which must render them acceptable to the responsible authorities of the various Governments.

Lastly, I would add that the good will which pervaded the meetings, the general success of the Congress as a whole, were in no small degree due to the genial influence of the single-hearted, earnest-minded man who convened it—Admiral Mouchez.

[D. G.]

GENERAL MONTHLY MEETING,

Monday, June 6, 1887.

Henry Pollock, Esq. Treasurer and Vice-President, in the Chair.

John Mowlem Burt, Esq.
James Staats Forbes, Esq.
Hugh Gordon, Esq. B.A. Oxon.
Douglas Hankey, Esq.
John Isaac Thornycroft, Esq. M. Inst. C.E.

were elected Members of the Royal Institution.

The following Address to the Queen was read and approved, and authorised to be signed by His Grace The President on behalf of the Members :—

To Her Most Gracious Majesty the Queen, Patron of the Royal Institution of Great Britain.

Madam,

We, the President and Members of the Royal Institution of Great Britain, in General Meeting assembled, humbly beg that your Majesty will be pleased to accept this our tribute of homage and congratulation on the occasion of the Jubilee of the commencement of your Majesty's reign—a reign signalised by so many blessings and distinguished for the encouragement and advancement of the Sciences for the promotion of which this our Institution was founded.

Further, it is our earnest desire and fervent prayer that your Majesty's reign in health, happiness, and prosperity may be prolonged for many years to come—a prayer echoed throughout the wide realms which own allegiance to your Majesty's Sceptre.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

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Industries for May, 1887. fol.
Iron for May, 1887. 4to.
Nature for May, 1887. 4to.
Revue Scientifique for May, 1887. 4to.
Scientific News for May, 1887. 4to.
Telegraphic Journal for May, 1887. 8vo.
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Gordon, Surgeon-General C. A. M.D. C.B. (*the Author*)—Inoculation for Rabies and Hydrophobia. 8vo. 1887.
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University Circular, No. 57. 4to. 1887.
Madrid Royal Academy of Sciences—Memorias, Tome XI. 4to. 1887.
Revista : Tome XXII. Nos. 2, 3. 8vo. 1887.
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Catalogue of Library, 1885, Supplement V. 8vo. 1887.
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WEEKLY EVENING MEETING,

Friday, June 10, 1887.

Henry Pollock, Esq. Treasurer and Vice-President, in the Chair.

Thomas Hodgkin, Esq. D.C.L.

Aquileia, the Precursor of Venice.

Aquileia, or Aglar, is now a little village, or rather a cluster of little villages, at the head of the Adriatic, just within the Austrian frontier, and with a population of about 2000. But it was at the time of the Christian era one of the chief cities of the Empire, covering an area of not less than 16 square miles, and with a population which cannot have been less than 100,000, and may have greatly exceeded that figure.

It was founded as a Roman colony 181 b.c., in order to keep the Gaulish tribes upon the north-east frontier of Italy in check and to prevent the danger of an alliance between them and the hostile king of Macedonia. The three Commissioners (Triumviri) for the settlement of the colony were Publius Scipio Nasica, Caius Flaminius, and Lucius Manlius Acidinus. The territory allotted to the new *Colonia* was 180 square miles in extent and it was occupied by 3000 foot-soldiers and some cavalry whose numbers are not stated. The place chosen for the city was about seven miles from the sea, on the banks of the river Natiso, which has, however, since changed its course and does not now flow near to the ruins of the city.

The name of the new city was derived either from the north wind (Aquilo), or according to another account, from an eagle, which on the day of the inauguration of the colony was seen suddenly to fly past the right hand of the statue of Jupiter, a most auspicious omen.

Somewhere about fifty years from the foundation of the colony its prosperity was enormously increased by the discovery of gold mines in the country of the Taurisci, a short distance to the north of it. Aquileia may thus be considered as, in a sense, the Melbourne of the Roman State. Seated too in the centre of a vast net-work of roads, she was admirably adapted to be the *entrepôt* of the commerce of Italy and Illyricum, of the Adriatic, and the rivers which flow into the Danube. On the west she was connected with Verona and Padua. On the north the two passes of the Pontebba and the Predil communicated with the Tyrol and Carinthia. Eastward the great roads to Laybach and to Trieste went forth to the long valley of the Save and the palace-bordered peninsula of Istria.

Aquileia was honoured in the year 11 b.c. by a visit from the

Emperor Augustus. His step-sons were waging war in the still half-civilised province of Pannonia, and the Emperor took up his head-quarters in the great city by the Natiso in order to guard their lines of communication and quicken the supply of provisions and munitions of war to their legions. He was accompanied by his wife Livia, who seems to have been extremely partial to this part of Italy, and who attributed her long life to her habitual use of the full-bodied "Pucine" wine which was produced from the vineyard of the Timavus.

At this visit of Augustus to Aquileia he was met by Herod the Great, who desired to obtain the Imperial sanction to the execution of Alexander and Aristobulus, his sons by the Asmodean princess Mariamne. They defended themselves, however, successfully, from the charge of conspiring against their father's life, and the domestic feud was for a time, but only for a time, composed by the influence of Augustus.

Under the Empire, Aquileia was chiefly famous for the number of sieges which it underwent, and (until the last of the series was reached) which it successfully resisted. The Marcomanni were repulsed from before its walls about the year 167. Maximin, the barbarian Emperor of Rome, who had been deposed by the Senate, blockaded the city for some time, but was slain by his mutinous soldiers (238) before he had succeeded in its capture. During this siege the ladies of Aquileia cut off their hair in order to supply the deficient stores of ropes for working the military engines. Their patriotism was commemorated after the close of the siege by the dedication of a temple "to the bald-headed Venus."

In 361, Julian besieged the city, which was defended by the troops of Constantius. The death of the latter emperor, which occurred while the siege was still pending, ended the war.

In 388, Theodosius pursued his defeated rival Maximus to the gates of Aquileia. The city was practically undefended, and Maximus, dragged forth to the camp of the conqueror at the third milestone from the city, was there put to death.

Lastly, in the year 452, Attila, with his terrible horde of Huns, dragging the chief Teutonic nations of Europe in their train, crossed the Julian Alps, and made his appearance before Aquileia. The inhabitants resisted with the energy of despair, and Attila, after a long blockade, was about to abandon the siege, when (according to a well-known legend) he looked up and saw the storks preparing to abandon the doomed city. Skilfully turning the natural omen to account, he rallied his soldiers for another and a desperate assault, which was successful. In his barbarous fury at the tenacity of the defence he put all the inhabitants, men, women, and children, to the sword, and then gave their city to the flames. According to a local legend, a great mound at the neighbouring city of Udine was raised by the Huns in order that their king might from the top of it the better behold the burning of Aquileia.

The barbarous destruction of Aquileia was the cause of the foundation of Venice. From Concordia, Altinum, Patavium, and all the towns and villages along the Adriatic shore the inhabitants fled in panic to the lugunes, and there between sea and sky, upon the little islands which cluster round the Rialto, they founded their new settlement which was one day to

> "hold the gorgeous east in fee,
> And be the safeguard of the west."

Aquileia never regained either her commercial or political import ance. Ecclesiastically, however, she was still held in honour, and her Patriarch was for centuries one of the most powerful spiritual rulers in the north of Italy. A kind of rival patriarchate was indeed established at Grado on the sea coast, under the protection of the Byzantine Emperors, while the Lombards on the mainland protected the Patriarch of Aquileia, but in the end Aquileia prevailed. The cathedral of Aquileia, which was raised in the tenth century, is a noble building in the Romanesque style, with some classical columns, probably taken from a heathen temple, with a grouping of seats for bishops and presbyters round the Patriarch, something like that which is seen at Torcello, with a crypt in which is the tomb of St. Hermagoras, the first bishop of Aquileia and alleged contemporary of the Apostles, and with a high gable-crowned campanile detached from the main body of the church.

The village, or to speak more accurately, the three villages which lurk among the ruins of Aquileia, are of a mean and squalid appearance, and as was said at the beginning of this paper, contain probably little more than a hundredth of the population of the ancient city.

[T. H.]

GENERAL MONTHLY MEETING,

Monday, July 4, 1887.

HENRY POLLOCK, Esq. Treasurer and Vice-President, in the Chair.

Henry Davey, Esq.
Harry Robert Graham, Esq.

were elected Members of the Royal Institution.

The following Letter was read:—

"WHITEHALL, 28*th June*, 1887.

"SIR,

"I have had the Honor to lay before The Queen the loyal and dutiful "Address of the President and Members of the Royal Institution of Great "Britain on the occasion of Her Majesty attaining the Fiftieth Year of Her "Reign;—And I have to inform you that Her Majesty was pleased to receive "the same very graciously.

"I have the Honor to be, Sir,
"Your obedient Servant,
"HENRY MATTHEWS.

"*To the* HON. SECRETARY OF THE
"ROYAL INSTITUTION OF GREAT BRITAIN."

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research:—

Ludwig Mond, Esq. £100.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XX. Part 2. 8vo. 1887.
The Secretary of State for India—Great Trigonometrical Survey of India, Vol. IV. A. 4to. 1886.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. III. Fasc. 8. 8vo. 1887.
American Academy of Arts and Sciences—Memoirs, Vol. XI. Part 4, No. 5. 4to. 1886.
Proceedings, Vol. XXII. Part 1. 8vo. 1887.
American Association for the Advancement of Science—Proceedings, Vols. XXXIV. XXXV. 8vo. 1886-7.
Asiatic Society, Royal (Bombay Branch)—Journal, Vol. XVIII. Extra Number, 1884-6. 8vo. 1887.
Astronomical Society, Royal—Monthly Notices, Vol. XLVII. No. 7. 8vo. 1887.
Australian Museum—Report for 1886, Supplement. fol. 1887.
Bankers, Institute of—Journal, Vol. VIII. Part 6. 8vo. 1887.
Bennett's Intelligence Association—Intelligence Quarterly for London and Suburbs, No. 1. 4to. 1887.
British Architects, Royal Institute of—Proceedings, 1886-7, No. 16. 4to.

British Association for the Advancement of Science—Report, 1886, Birmingham. 8vo. 1887.
Chemical Society—Journal for June, 1887. 8vo.
Chief Signal Officer, U.S. Army—Annual Report for 1885, Parts 1 and 2. 8vo. 1885.
Chile, Officina Central Meteorolojica—Anuario, 1886, No. 5. 8vo. 1887.
Churchill, Messrs. J. and A. (the Publishers)—Journal of Laryngology and Rhinology, No. 6. 8vo. 1887.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, 1887, Part 3. 8vo.
Dax, Société de Borda—Bulletin, 2e Serie, Douzième Année, Trimestre 2e. 8vo. 1887.
Editors—American Journal of Science for June, 1887. 8vo.
Analyst for June, 1887. 8vo.
Athenæum for June, 1887. 4to.
Chemical News for June, 1887. 4to.
Chemist and Druggist for June, 1887. 8vo.
Engineer for June, 1887. fol.
Engineering for June, 1887. fol.
Horological Journal for June, 1887. 8vo.
Industries for June, 1887. fol.
Iron for June, 1887. 4to.
Murray's Magazine for June, 1887. 8vo.
Nature for June, 1887. 4to.
Revue Scientifique for June, 1887. 4to.
Scientific News for June, 1887. 4to.
Telegraphic Journal for June, 1887. 8vo.
Zoophilist for June, 1887. 4to.
Engineering, Editor of—Cable or Rope Traction. By J. B. Smith. 4to. 1887.
Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 34, 35. 8vo. 1887.
Franklin Institute—Journal, No. 738. 8vo. 1887.
Geographical Society, Royal—Proceedings, New Series, Vol. IX. No. 6. 8vo. 1887.
Harlem, Société Hollandaise des Sciences—Archives Neerlandaises, Tome XXI. Liv. 5. 8vo. 1887.
Natuurkundige Verhandelingen, 3de Verz, Deel IV. 4de Stuk. 4to. 1887.
Johns Hopkins University—American Journal of Philology, Vol. VIII. No. 1. 8vo. 1887.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quinta, Vol. I. No. 3. 8vo. And Disegni. fol. 1887.
Norwegischen Commission der Europaischen Gradmessung—Geodätische Arbeiten, Heft 5. 4to. 1887.
Vandstandsobservationer. Heft 4. 4to. 1887.
Odontological Society of Great Britain—Transactions, Vol. XIX. No. 7. New Series. 8vo. 1887.
Pharmaceutical Society of Great Britain—Journal, June, 1887. 8vo.
Photographic Society—Journal, Vol. XI. No. 8. 8vo. 1887.
Physical Society of London—Proceedings, Vol. VIII. Part 4. 8vo. 1887.
Royal Society of London—Proceedings, Nos. 254, 255. 8vo. 1887.
Royal Dublin Society—Transactions, Vol. III. Nos. 11–13. 4to. 1886–7.
Proceedings, Vol. V. Parts 3–6. 8vo. 1886–7.
St. Petersbourg, Academie Impériale des Sciences—Bulletin, Tome XXXI. No. 4. 4to. 1887.
Saxon Society of Sciences, Royal—Philologisch-historische Classe: Abhandlungen, Band X. No. 4. 8vo. 1887.
Mathematisch-physische Classe: Abhandlungen, Band XIII. Nos. 8, 9. 8vo. 1887.
Smithsonian Institution—Fourth Report of the Bureau of Ethnology, 1882–3. 4to. 1886.

Society of Arts—Journal, June, 1887. 8vo.
Tasmania, Royal Society of—Papers and Proceedings for 1886. 8vo. 1887.
United Service Institution, Royal—Journal, No. 139. 8vo. 1887.
United States Geological Survey—Monograph X. 4to. 1886.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1887: Heft 5. 4to.
Victoria Institute—Journal of Transactions, No. 81. 8vo. 1887.
Yale University, New Haven, U.S.—Transactions of the Astronomical Observatory, Vol. I. Part 1. 4to. 1887.
Zoological Society of London—Proceedings, 1887, Part 1. 8vo.

GENERAL MONTHLY MEETING,

Monday, November 7, 1887.

WARREN DE LA RUE, Esq. D.C.L. F.R.S. Manager and Vice-President, in the Chair.

Edmund Gosse, Esq. M.A.
F. H. Lewis, Esq.

were elected Members of the Royal Institution.

The Managers reported that they had admitted Mr. Herbert Taylor as an Annual Subscriber of the Royal Institution.

The Special Thanks of the Members were returned to Mr. W. Resbury Few for his present of a large Photograph of the Nebulæ of Orion, the property of his late father, Mr. Charles Few, *M.R.I.*

The Special Thanks of the Members were returned to Mr. William Anderson, *M.R.I.* for his present of a Double Acting High Vacuum Pump.

The Special Thanks of the Members were returned to the Rev. John Macnaught, *M.R.I.* for his present of £50 for improvements in the Lecture Theatre.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Observations for 1885. 4to. 1887.
Greenwich Spectroscopic and Photographic Results, 1885. 4to. 1887.
Numerical Lunar Theory. By Sir G. B. Airy. 4to. 1886.
Report on Observations of the Transit of Venus, 1882. fol. 1887.
Academy of Natural Sciences, Philadelphia—Proceedings, 1887, Part 1. 8vo.
Accademia dei Lincei, Reale, Roma—Atti della Classe di Scienze Morali, Storiche e Filologiche—Serie 4ª, Vol. II. Parte 2°. 4to. 1886–7.
Atti, Serie Quarta: Rendiconti. Vol. III. 1° Semestre, Fasc. 10–13; 2° Semestre, Fasc. 1, 2, 3. 8vo. 1887.
American Philosophical Society—Proceedings, No. 125. 8vo. 1887.
Antiquaries, Society of—Proceedings, Vol. XI. No. 3. 8vo. 1887.
Ashburner, Charles A. Esq. (the Author)—The Geologic Distribution of Natural Gas in the United States. 8vo. 1886.
The Geologic Relations of the Nantichoke Disaster. 8vo. 1887.
Asiatic Society, Royal—Journal, Vol. XIX. Part 4. 8vo. 1887.
Asiatic Society of Bengal—Journal, Vol. LVI. Part 1. No. 1. 8vo. 1887.
Proceedings, 1887, Nos. 2–5. 8vo.
Asiatic Society, Royal (China Branch)—Journal, Vol. XXI. Nos. 5, 6. 8vo. 1886.
Astronomical Society, Royal—Monthly Notices, Vol. XLVII. No. 8. 8vo. 1887.

Australian Museum—Report for 1887. fol. 1887.
Baker, Benjamin, Esq. M. Inst. C.E. M.R.I. (the Author)—Bridging the Firth of Forth. With Illustrations. [Lecture delivered at the Royal Institution.] 4to. 1887.
Bankers, Institute of—Journal, Vol. VIII. Part 7. 8vo. 1887.
Bavarian Academy of Sciences—Abhandlungen, Band XVI. Abtheilung 1. 4to. 1887.
Sitzungsberichte, 1886, Heft 1–3; 1887, Heft 1; Inhaltsverzeichniss, 1871–85. 8vo. 1886–7.
Gedächtnisrede auf J. von Fraunhofer. 4to. 1887.
Belgique, Académie des Sciences, &c.—Mémoires, Tome XLVI. 4to. 1886.
Mémoires Couronnées, Tomes XLVII. XLVIII. 4to. 1886. Tomes XXXVI. XXXVIII. XXXIX. 8vo. 1886.
Bulletins, 3e Serie, Tomes IX.–XIII. 8vo. 1885–7.
Annuaires, 1886 and 1887. 16to.
Notices Biographiques, &c. 1886. 16to. 1887.
Catalogue des Livres de la Bibliothèque, Partie 1, 2. 8vo. 1881–7.
British Architects, Royal Institute of—Proceedings, 1886–7, Nos. 17, 18; 1887–8. No. 1. 4to.
Kalendar, 1887–8. 8vo.
Brymner, Douglas, Esq. (Archivist)—Report on Canadian Archives, 1886. 8vo. 1887.
Canada Meteorological Service—Report, 1884. 8vo. 1887.
Chemical Society—Journal for July-Oct. 1887. 8vo.
Churchill, Messrs. J. and A. (the Publishers)—Journal of Laryngology and Rhinology, Nos. 7–10. 8vo. 1887.
City of London College—Calendar, 1887–8. 8vo. 1887.
Civil Engineers' Institution—Minutes of Proceedings, Vols. LXXXIX, XC. 8vo. 1886–7.
List of Members. 8vo. 1887.
Clinical Society—Transactions, Vol. XX. 8vo. 1887.
Cornwall Polytechnic Society, Royal—Annual Report, 1886. 8vo. 1887.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, 1887, Parts 4, 5. 8vo.
Dax, Société de Borda—Bulletin, 2e Serie, Douzième Année, Trimestre 3e. 8vo. 1887.
East India Association—Journal, Vol. XIX. Nos. 5, 6, 7. 8vo. 1887.
Editors—American Journal of Science for July–Oct. 1887. 8vo.
Analyst for July–Oct. 1887. 8vo.
Athenæum for July–Oct. 1887. 4to.
Chemical News for July–Oct. 1887. 4to.
Chemist and Druggist for July–Oct. 1887. 8vo.
Engineer for July–Oct. 1887. fol.
Engineering for July–Oct. 1887. fol.
Horological Journal for July–Oct. 1887. 8vo.
Industries for July–Oct. 1887. fol.
Iron for July–Oct. 1887. 4to.
Murray's Magazine for July–Oct. 1887. 8vo.
Nature for July–Oct. 1887. 4to.
Revue Scientifique for July–Oct. 1887. 4to.
Scientific News for July–Oct. 1887. 4to.
Telegraphic Journal for July–Oct. 1887. 8vo.
Zoophilist for July–Oct. 1887. 4to.
"*Engineering*," *Editor of*—The Metallurgy of Silver, Gold, and Mercury in the United States. By T. Egleston. Vol. I. 8vo. 1887.
Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 36–42. 8vo. 1887.
Codici Palatini, Vol. I. Fasc. 6. 8vo. 1887.
Foreign Office—Catalogue of Printed Books in the Library, 31 Dec. 1885. 4to. 1887.

Franklin Institute—Journal, Nos. 739–742. 8vo. 1887.
Geographical Society, Royal—Proceedings, New Series, Vol. IX. Nos. 7–10. 8vo. 1887.
Geological Institute, Imperial, Vienna—Jahrbuch, Band XXXVII. Heft 1. 8vo. 1887.
Verhandlungen, 1887, Nos. 2–8. 8vo. 1887.
Geological Society—Quarterly Journal, No. 171. 8vo. 1887.
Georgofili, Reale Academia—Atti, Vol. IX. Sup.; Vol. X. Disp. 1, 2. 8vo. 1886–7.
Harlem, Société Hollandaise des Sciences—Archives Neerlandaises, Tome XXII. Liv. 1. 8vo. 1887.
Iron and Steel Institute—Journal, 1887, No. 1. 8vo.
Johns Hopkins University—American Journal of Philology, Vol. VIII. No. 2. 8vo. 1887.
American Chemical Journal, Vol. IX. Nos. 3, 4, 5. 8vo. 1887.
Studies in Historical and Political Science, Fifth Series, Nos. 8, 9. 8vo. 1887.
University Circular, Nos. 58, 59. 4to. 1887.
Lea, M. Carey, Esq. (*the Author*)—Photochemistry of the Silver Haloids. 8vo. 1887.
Linnean Society—Journal, Nos. 117, 129, 149, 158, 159. 8vo. 1887.
Transactions, Zoology Vol. IV. Parts I and 2; Botany Vol. II. Parts 9–14. 4to. 1886–7.
Proceedings, Nov. 1883 to June 1887. 8vo. 1886–7.
Lisbon, Royal Academy of Sciences—Memorias, Nova Serie, Tomo V. Parte 2; Tomo VI. Parte 1. 4to. 1882–5.
Jornal, Num. XXX.–XLIV. 8vo. 1881–7.
Curso de Silvicultura, Tomo I. 8vo. 1886.
Manchester Geological Society—Transactions, Vol. XIX. Parts 8–10. 8vo. 1887.
Manila, Universidad de Sto. Tomas—Discurso por R. P. F. R. Velazquez y Conde. 4to. 1887.
Maryland Medical and Chirurgical Faculty—Transactions, 1887. 8vo.
Mechanical Engineers' Institution—Proceedings, 1887, No. 2. 8vo.
Medical and Chirurgical Society, Royal—Proceedings, No. 16. 8vo. 1887.
Transactions, Vol. LXX. 8vo. 1887.
Meteorological Office—Hourly Readings, 1884, Part 4. 4to. 1887.
Monthly Weather Report for Dec. 1886. 4to.
Quarterly Weather Reports, 1878, Part 4; 1879, Part 1. 4to. 1887.
Weekly Weather Reports, Vol. IV, Nos. 12–33. 4to. 1887.
Meteorological Society, Royal—Quarterly Journal, Nos. 62, 63. 8vo. 1887.
Meteorological Record, Nos. 24–26. 8vo. 1887.
Hints to Meteorological Observers. By W. Marriott. 8vo. 1887.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quinta. Vol. I. Nos. 4–8. 8vo. And Disegni. fol. 1887.
National Fish Culture Association—Journal, Vol. I. No. 3. 4to. 1887.
New South Wales Government—Year Book of New South Wales, 1887. 8vo.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXVI. Parts 3, 4. 8vo. 1887.
Numismatic Society—Chronicle and Journal, 1887, Parts 1, 2. 8vo. 1887.
Odontological Society of Great Britain—Transactions, Vol. XIX. No. 8. New Series. 8vo. 1887.
Perry, Rev. S. J. F.R.S. (*the Author*)—Results of Meteorological and Magnetical Observations, 1886. 12mo. 1887.
Pharmaceutical Society of Great Britain—Journal, July–Oct. 1887. 8vo.
Photographic Society—Journal, Vol. XI. No. 9; Vol. XII. No. 1. 8vo. 1887.
Preussische Akademie der Wissenschaften—Sitzungsberichte XIX.–XXXIX. 8vo. 1887.
Radcliffe Observatory—Radcliffe Observations for 1884. 8vo. 1887.
Richardson, B. W. M.D. F.R.S. (*the Author*)—The Asclepiad, Vol. IV. No. 15. 8vo. 1887.
Rio de Janeiro Observatory—Revista, Nos. 5, 6, 8, 9. 8vo. 1887.

Royal College of Surgeons of England—Calendar, 1887. 8vo.
Royal Society of Canada—Proceedings and Transactions, Vol. IV. 4to. 1887.
Royal Society of London—Proceedings, Nos. 256, 257, 258. 8vo. 1887.
Saxon Society of Sciences, Royal—Philologisch-historische Classe: Abhandlungen, Band X. No. 5. 8vo. 1887.
Berichte, 1887, Nos. 1–3. 8vo. 1887.
Mathematisch-physische Classe: Abhandlungen, Band XIV. Nos. 1–4. 8vo. 1887.
Smith, Basil Woodd, Esq. M.R.I.—Middlesex County Records, Vol. II. Edited by J. C. Jeaffreson. 8vo. 1887.
Smithsonian Institution—Scientific Writings of Joseph Henry. 2 vols. 4to. 1886.
Annual Report for 1885, Part 1. 8vo. 1886.
Smithsonian Miscellaneous Collections, Vols. XXVIII. XXIX. XXX. 8vo. 1887.
Society of Arts—Journal, July-Oct. 1887. 8vo.
Statistical Society—Journal, Vol. L. Part 3. 8vo. 1887.
Telegraph Engineers, Society of—Journal, No. 67. 8vo. 1887.
Tidy, Charles Meymott, Esq. M.B. F.C.S. M.R.I. (the Author)—Handbook of Modern Chemistry. Inorganic and Organic. 2nd ed. 8vo. 1887.
United Service Institution, Royal—Journal, No. 140. 8vo. 1887.
United States Geological Survey—Bulletins, Nos. 34–39. 8vo. 1886–7.
Venezuela, Consul for—Correspondence between the Venezuelan Government and H.B.M.'s Government. fol. 1887.
Vereins zur Beförderung des Gewerbfleisses in Preusen—Verhandlungen, 1887: Heft 6, 7. 4to.
Victoria Institute—Journal of Transactions, No. 82. 8vo. 1887.
Wagner Free Institute of Science, Philadelphia—Transactions, Vol. I. 8vo. 1887.
Yorkshire Archæological and Topographical Association—Journal, Part 38. 8vo. 1887.
Zoological Society of London—Proceedings, 1887, Parts 2, 3. 8vo.

Royal Institution of Great Britain.

GENERAL MONTHLY MEETING,

Monday, December 5, 1887.

WARREN DE LA RUE, Esq. M.A. D.C.L. F.R.S. Vice-President, in the Chair.

Victor Horsley, Esq. F.R.S. F.R.C.S.
Mrs. Victor Horsley,
George Lindsay Johnson, Esq. M.B. M.A. F.R.C.S.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Sir HENRY DOULTON, M.R.I. for his present of Two "Doulton" Fireplaces.

The following Lecture Arrangements were announced:—

SIR ROBERT STAWELL BALL, LL.D. F.R.S. Royal Astronomer of Ireland.—Six Lectures (adapted to a Juvenile Auditory) on ASTRONOMY: The Sun, Moon, Planets, Comets, and Stars. On Dec. 27 (*Tuesday*), Dec. 29, 31, 1887; Jan. 3, 5, 7, 1888.

GEORGE JOHN ROMANES, Esq. M.A. LL.D. F.R.S. *M.R.I.*—Ten Lectures, "BEFORE AND AFTER DARWIN." On *Tuesdays*, Jan. 17 to March 20.

HUBERT HERKOMER, Esq. M.A. A.R.A. Slade Professor of Fine Art in the University of Oxford.—Three Lectures on *Thursdays*, Jan. 19, THE WALKER SCHOOL; Jan. 26, MY VISITS TO AMERICA; Feb. 2, ART EDUCATION.

C. HUBERT H. PARRY, Esq. M.A. Professor of Musical History and Composition at the Royal College of Music.—Four Lectures on EARLY SECULAR CHORAL MUSIC, from the Thirteenth Century till the beginning of the Seventeenth. (With Illustrations.) On *Thursdays*, Feb. 9, 16, 23, March 1.

THE REV. W. H. DALLINGER, LL.D. F.R.S.—Three Lectures on MICROSCOPICAL WORK WITH RECENT LENSES ON THE LEAST AND SIMPLEST FORMS OF LIFE. On *Thursdays*, March 8, 15, 22.

THE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*—Seven Lectures on EXPERIMENTAL OPTICS (Illustrated by Electric Light). On *Saturdays*, Jan. 21 to March 3.

WILLIAM ARCHER, Esq.—Three Lectures on THE MODERN DRAMA: French, Scandinavian, and English. On *Saturdays*, March 10, 17, 24.

THE PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XX. Part 3. 8vo. 1887.
The New Zealand Government—Results of a Census of the Colony of New Zealand, 28 March, 1886. fol. 1887.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. III. 2° Semestre, Fasc. 4, 5. 8vo. 1887.
Agricultural Society of England, Royal—Journal, Second Series, Vol. XXIII. Part 2. 8vo. 1887.
Antiquaries, Society of—Archæologia, Vol. L. Part 2. 4to. 1887.
Astronomical Society, Royal—Monthly Notices, Vol. XLVII. No. 9. 8vo. 1887.
Bankers, Institute of—Journal, Vol. VIII. Part 8. 8vo. 1887.

British Architects, Royal Institute of—Proceedings, 1887–8, Nos. 2, 3. 4to.
Cambridge Philosophical Society—Proceedings, Vol. VI. Part 2. 8vo. 1887.
Chemical Society—Journal for November, 1887. 8vo.
Churchill, Messrs. J. and A. (the Publishers)—Journal of Laryngology and Rhinology, No. 11. 8vo. 1887.
Devonshire Association for the Advancement of Science, Literature, and Art—Report and Transactions, Vol. XIX. 8vo. 1887.
The Devonshire Domesday, Part 4. 8vo. 1887.
Editors—American Journal of Science for November, 1887. 4to.
Analyst for November, 1887. 8vo.
Architects' Register. 8vo. 1887.
Athenæum for November, 1887. 4to.
Chemical News for November, 1887. 4to.
Chemist and Druggist for November, 1887. 8vo.
Engineer for November, 1887. fol.
Engineering for November, 1887. fol.
Horological Journal for November, 1887. 8vo.
Industries for November, 1887. fol.
Iron for November, 1887. 4to.
Murray's Magazine for November, 1887. 8vo.
Nature for November, 1887. 4to.
Revue Scientifique for November, 1887. 4to.
Scientific News for November, 1887. 4to.
Telegraphic Journal for November, 1887. 8vo.
Zoophilist for November, 1887. 4to.
Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 44, 45. 8vo. 1887.
Franklin Institute—Journal, No. 743. 8vo. 1887.
Geographical Society, Royal—Proceedings, New Series, Vol. IX. No. 11. 8vo. 1887.
Glasgow Philosophical Society—Proceedings, Vol. XVIII. 8vo. 1887.
Historical Society, Royal—England and Napoleon in 1803: Despatches of Lord Whitworth and others. Edited by Oscar Browning. 8vo. 1887.
Lee, Henry, Esq. M.R.I. (the Author)—On the Tapetum Lucidum and the Function of the Fourth Pair of Nerves. 8vo. 1887.
Madras Government Central Museum—Report, 1886–7. fol.
Meteorological Office—Hourly Readings, 1885, Part 1. 4to.
Quarterly Weather Report, 1879, Part 2. 4to.
Middlesex Natural History and Science Society—Transactions, 1886–7. 8vo. 1887.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quinta, Vol. I. Nos. 9, 10. 8vo. And Disegni. fol. 1887.
Pharmaceutical Society of Great Britain—Journal, November, 1887. 8vo.
Radcliffe Library, Oxford—Catalogue of the Transactions of Societies, Periodicals, and Memoirs. 4th Ed. 8vo. 1887.
Richardson, B. W. M.D. F.R.S. (the Author)—The Asclepiad, Vol. IV. No. 16. 8vo. 1887.
Rio de Janeiro Observatory—Revista, No. 10. 8vo. 1887.
St. Petersbourg, Academie Impériale des Sciences—Memoires, Tome XXXV. Nos. 2, 3. 4to. 1887.
Saxon Society of Sciences, Royal—Philologisch-historische Classe: Abhandlungen, Band X. No. 6. 8vo. 1887.
Society of Arts—Journal, November, 1887. 8vo.
Teyler Museum, Haarlem—Archives Ser. II. Vol. III. 1e Partie. 4to. 1887.
Catalogue de la Bibliothéque, Liv. 5, 6. 4to. 1886.
United Service Institution, Royal—Journal, No. 141. 8vo. 1887.
Index to Journal, Vols. XXI.–XXX. 8vo. 1887.
Brief History of the Institution. By Capt. B. Burgess. 8vo. 1887.
United States Geological Survey—First Annual Report. 4to. 1880.
Upsal, Royal Society of Sciences—Nova Acta, Ser. III. Vol. XIII. Fasc. 2. 4to. 1887.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1887: Heft 8. 4to.

WEEKLY EVENING MEETING,

Friday, January 20, 1888.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*
PROFESSOR OF NATURAL PHILOSOPHY, R.I.

Diffraction of Sound.

The interest of the subject which I propose to bring before you this evening turns principally upon the connection or analogy between light and sound. It has been known for a very long time that sound is a vibration; and every one here knows that light is a vibration also. The last piece of knowledge, however, was not arrived at so easily as the first; and one of the difficulties which retarded the acceptance of the view that light is a vibration was that in some respects the analogy between light and sound seemed to be less perfect than it should be. At the present time many of the students at our schools and universities can tell glibly all about it; yet this difficulty is one not to be despised, for it exercised a determining influence over the great mind of Newton. Newton, it would seem, definitely rejected the wave theory of light on the ground that according to such a theory light would turn round the corners of obstacles, and so abolish shadows, in the way that sound is generally supposed to do. The fact that this difficulty seemed to Newton to be insuperable is, from the point of view of the advancement of science, very encouraging. The difficulty which stopped Newton two centuries ago is no difficulty now. It is well known that the question depends upon the relative wave-lengths in the two cases. Light-shadows are sharp under ordinary circumstances, because the wave-length of light is so small: sound-shadows are usually of a diffused character, because the wave-length of sound is so great. The gap between the two is enormous. I need hardly remind you that the wave-length of C in the middle of the musical scale is about 4 feet. The wave-length of the light with which we are usually concerned, the light towards the middle of the spectrum, is about the forty-thousandth of an inch. The result is that an obstacle which is immensely large for light may be very small for sound, and will therefore behave in a different manner.

That light-shadows are sharp is a familiar fact, but as I can prove it in a moment I will do so. We have here light from the electric arc thrown on the screen; and if I hold up my hand thus we have a sharp shadow at any moderate distance, which shadow can be made

sharper still by diminishing the source of light. Sound-shadows, as I have said, are not often sharp; but I believe that they are sharper than is usually supposed, the reason being that when we pass into a sound-shadow—when, for example, we pass into the shade of a large obstacle, such as a building—it requires some little time to effect the transition, and the consequence is that we cannot make a very ready comparison between the intensity of the sound before we enter and its diminution afterwards. When the comparison is made under more favourable conditions, the result is often better than would have been expected. It is, of course, impossible to perform experiments with such obstacles before an audience, and the shadows which I propose to show you to-night are on a much smaller scale. I shall take advantage of the sensitiveness of a flame such as Professor Tyndall has often used here—a flame sensitive to the waves produced by notes so exceedingly high as to be inaudible to the human ear. In fact, all the sounds with which I shall deal to-night will be inaudible to the audience. I hope that no quibbler will object that they are therefore not sounds: they are in every respect analogous to the vibrations which produce the ordinary sensations of hearing.

I will now start the sensitive flame. We must adjust it to a reasonable degree of sensitiveness. I need scarcely explain the mechanism of these flames, which you know are fed from a special gasholder supplying gas at a high pressure. When the pressure is too high, the flame flares on its own account (as this one is doing now), independently of external sound. When the pressure is somewhat diminished, but not too much so—when the flame "stands on the brink of the precipice," were, I think, Tyndall's words—the sound pushes it over, and causes it to flare; whereas, in the absence of such sound, it would remain erect and unaffected. Now, I believe, the flame is flaring under the action of a very high note that I am producing here. That can be tested in a moment by stopping the sound, and seeing whether the flame recovers or not. It recovers now. What I want to show you, however, is that the sound shadows may be very sharp. I will put my hand between the flame and the source of sound, and you will see the difference. The flame is at present flaring; if I put my hand here, the flame recovers. When the adjustment is correct, my hand is a sufficient obstacle to throw a most conspicuous shadow. The flame is now in the shadow of my hand, and it recovers its steadiness: I move my hand up, the sound comes to the flame again, and it flares. When the conditions are at their best, a very small obstacle is sufficient to make the entire difference, and a sound shadow may be thrown across several feet from an obstacle as small as the hand. The reason of the divergence from ordinary experience here met with is, that while the hand is a fairly large obstacle in comparison with the wave-length of the sound I am here using, it would not be a sufficiently large obstacle in comparison with the wave-lengths with which we have to do in ordinary life and in music.

Everything then turns upon the question of the wave-length. The wave-length of the sound that I am using now is about half an inch. That is its complete length, and it corresponds to a note that would be very high indeed on the musical scale. The wave-length of middle C being four feet, the C one octave above that is two feet; two octaves above, one foot; three octaves above, six inches; four octaves, three inches; five octaves, one and a half inch; six octaves, three-quarters of an inch; between that and the next octave, that is to say, between six and seven octaves above middle C, is the pitch of the note that I was just now using. There is no difficulty in determining what the wave-length is. The method depends upon the properties of what are known as stationary sonorous waves as opposed to progressive waves. If a train of progressive waves are caused to impinge upon a reflecting wall, there will be sent back or reflected in the reverse direction a second set of waves, and the co-operation of these two sets of waves produces one set or system of stationary waves, the distinction being that, whereas in the one set the places of greatest condensation are continually changing and passing through every point, in the stationary waves there are definite points for the places of greatest condensation (nodes), and other distinct and definite (loops) for the places of greatest motion. The places of greatest variation of density are the places of no motion: the places of greatest motion are places of no variation of density. By the operation of a reflector, such as this board, we obtain a system of stationary waves, in which the nodes and loops occupy given positions relatively to the board.

You will observe that as I hold the board at different distances behind, the flame rises and falls—I can hardly hold it still enough. In one position the flame rises, further off it falls again; and as I move the board back the flame passes continually from the position of the node—the place of no motion—to the loop or place of greatest motion and no variation of pressure. As I move back the aspect of the flame changes; and all these changes are due to the reflection of the sound-waves by the reflector which I am holding. The flame alternately ducks and rises, its behaviour depending upon the different action of the nodes and loops. The nodes occur at distances from the reflecting wall, which are even multiples of the quarter of a wave-length; the loops are, on the other hand, at distances from the reflector which are odd multiples, bisecting therefore the positions between the loops. I will now show you that a very slight body is capable of acting as a reflector. This is a screen of tissue paper, and the effect will be apparent when it is held behind the flame and the distances are caused to vary. The flame goes up and down, showing that a considerable proportion of the sonorous intensity incident upon the paper screen is reflected back upon the flame; otherwise the exact position of the reflector would be of no moment. I have here, however, a different sort of reflector. This is a glass plate—I use glass so that those behind may see through it—and it

will slide upon a stand here arranged for it. When put in this position the flame is very little affected; the place is what I call a node—a place where there is great pressure variation, but no vibratory velocity. If I move the glass back, the flame becomes vigorously excited; that position is a loop. Move it back still more and the flame becomes fairly quiet; but you see that as the plate travels gradually along, the flame goes through these evolutions as it occupies in succession the position of a node or the position of a loop. The interest of this experiment for our present purpose depends upon this —that the distances through which the glass plate, acting as a reflector, must be successively moved in order to pass the flame from a loop to the next loop, or from a node to the consecutive node, is in each case half the wave length; so that by measuring the space through which the plate is thus withdrawn one has at once a measurement of the wave length, and consequently of the pitch of the sound, though one cannot hear it.

The question of whether the flame is excited at the nodes or at the loops,—whether at the places where the pressure varies most or at those where there is no variation of pressure, but considerable motion of air—is one of considerable interest from the point of view of the theory of these flames. The experiment could be made well enough with such a source of sound as I am now using; but it is made rather better by using sounds of a lower pitch and therefore of greater wave-length, the discrimination being then more easy. Here is a table of the distances which the screen must be from the flame in order to give the maximum and the minimum effect, the minimum being practically nothing at all.

Table of Maxima and Minima.

Max.	Min.
1·1	
	3·0
4·5	
	5·9
7·5	
	8·9
10·3	
	11·7
13·0	
	14·7
15·9	

The distance between successive maxima or successive minima is very nearly 3 (centims), and this is accordingly half the length of the wave.

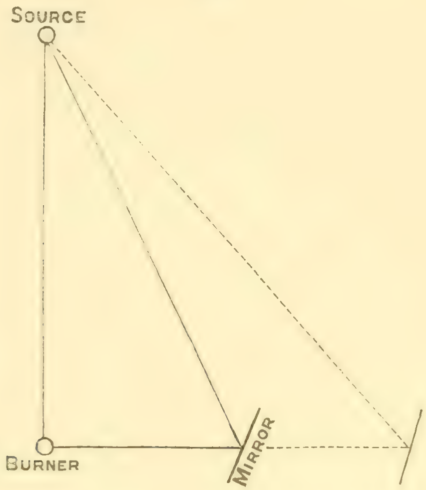
But there is a further question behind. Is it at the loops or is it at the nodes that the flame is most excited? The table shows what the answer must be, because the nodes occur at distances from the screen which are even multiples, and the loops at distances which are odd multiples; and the numbers in the table can be explained in only one way—that the flame is excited at the loops

corresponding to the odd multiples, and remains quiescent at the nodes corresponding to the even multiples. This result is especially remarkable, because the ear, when substituted for the flame, behaves in the exactly opposite manner, being excited at the nodes and not at the loops. The experiment may be tried with the aid of a tube, one end of which is placed in the ear, while the other is held close to the burner. It is then found the ear is excited the most when the flame is excited least, and *vice versâ*. The result of the experiment shows, moreover, that the manner in which the flame is disintegrated under the action of sound is not, as might be expected, symmetrical in regard to the axis of the flame. If it were symmetrical, it would be most affected by the symmetrical cause, namely, the variation of pressure. The fact being that it is most excited at the loop, where there is the greatest vibratory velocity, shows that the method of disintegration is unsymmetrical, the velocity being a directed quantity. In that respect the theory of these flames is different from the theory of the water-jets investigated by Savart, which resolve themselves into detached drops under the influence of sonorous vibration. The analogy fails at this point, and it has been pressed too far by some experimenters on the subject. Another simple proof of the correctness of the result of our experiment is that it makes all the difference which way the burner is turned in respect of the direction in which the sound-waves are impinging upon it. If the phenomenon were symmetrical, it would make no difference if the flame were turned round upon its vertical axis. But we find that it does make a difference. This is the way in which I was using the flame, and you see that it is flaring strongly. If I now turn the burner round through a right angle, the flame stops flaring. I have done nothing more than turn the burner round and the flame with it, showing that the sound-waves may impinge in one direction with great effect, and in another direction with no effect. The sensitiveness occurs again when the burner is turned through another right angle; after three right angles there is another place of no effect; and after a complete revolution of the flame the original sensitiveness recurs. So that if the flame were stationary, and the sound-waves came, say, from the north or south, the phenomena would be exhibited; but if they came from the east or west, the flame would make no response.

This is of convenience in experimenting, because, by turning the burner round, I make the flame almost insensitive to a sound, and I am now free to show the effect of any sound that may be brought to it in the perpendicular direction. I am going to use a very small reflector—a small piece of looking-glass. Wood would do as well; but looking-glass facilitates the adjustment, because my assistant, by seeing the reflection, will be able to tell me when I am holding it in the best position. Now, the sound is being reflected from the bit of glass, and is causing the flame to flare, though the same sound, travelling a shorter distance and impinging in another direction, is incompetent to produce the result (Fig. 1).

I am now going to move the reflector to and fro along the line perpendicular to that joining the source and the burner, all the while maintaining the adjustment, so that from the position of the source of sound the image of the flame is seen in the centre of the mirror. Seen from the source, it is still as central as before, but it has lost its effect, and as I move it to and fro I produce cycles of effect and no

Fig. 1.

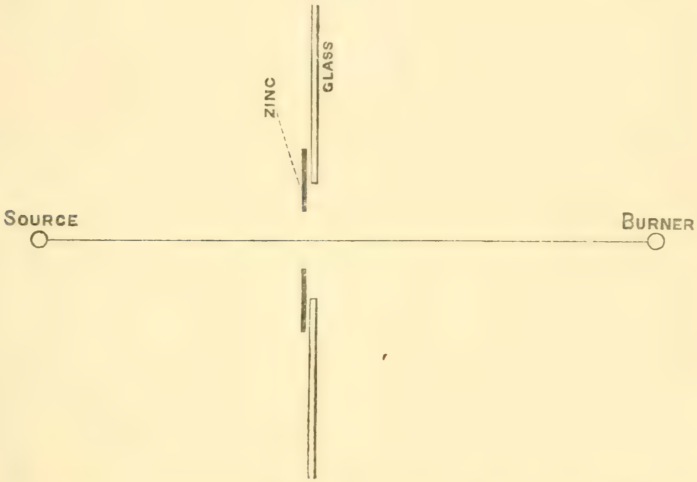


effect. What is the cause of this? The question depends upon something different from what I have been speaking of hitherto; and the explanation is, that we are here dealing with a diffraction phenomenon. The mirror is a small one, and the sound-waves which it reflects are not big enough to act in the normal manner. We are really dealing with the same sort of phenomena as arise in optics when we use small pin-holes for the entrance of our light. It is not very easy to make the experiment in the present form quite simple, because the mirror would have to be withdrawn, all the while maintaining a somewhat complicated adjustment. In order to raise the question of diffraction in its simplest shape, we must have a direct course for the sound between its origin and the place of observation, and interpose in the path a screen perforated with such holes as we desire to try.

The screen I propose to use is of glass. It is a practically perfect obstacle for such sounds as we are dealing with; but it is perforated here with a hole (20 cm. diameter), rendered more evident to those at a distance by means of a circle of paper pasted round it. The edge of the hole corresponds to the inner circumference of the paper. We shall thus be able to try the effect of different sized apertures, all the other circumstances remaining unchanged. The experi-

ment is rather a difficult one before an audience, because everything turns on getting the exact adjustment of distances relatively to the wave-length. At present the sound is passing through this comparatively large hole in the glass screen, and is producing, as you see, scarcely any effect upon the flame situated opposite to its centre. But if (Fig. 2) I diminish the size of the hole by holding this circle of zinc (perforated with a hole 14 cm. in diameter) in front of it, it is seen that, although the hole is smaller, we get a far greater effect. That

FIG. 2.

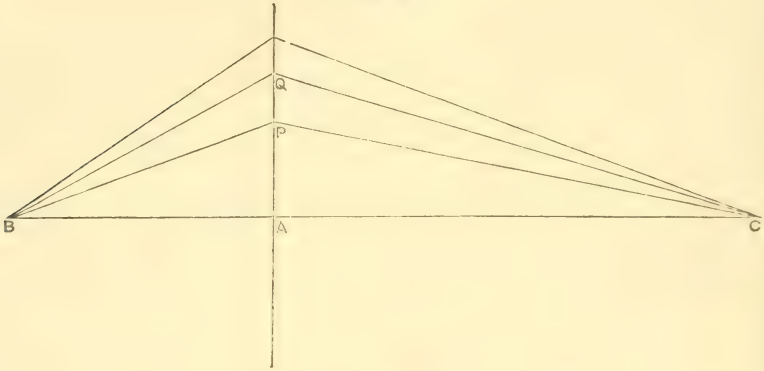


is a fundamental phenomenon in diffraction. Now I reopen the larger hole, and the flame becomes quiet. So that it is evident that in this case the sound produces a greater effect in passing through a small hole than in passing through a larger one. The experiment may be made in another way, by obstructing the central in place of the marginal part of the aperture in the glass. When I hold this unperforated disc of zinc (14 cm. in diameter) centrically in front, we get a greater effect than when the sound is allowed to pass through both parts of the aperture. The flame is now flaring vigorously under the action of the sonorous waves passing the marginal part of the aperture, whereas it will scarcely flare at all under the action of waves passing through both the marginal and the central hole.

This is a point which I should like to dwell upon a little, for it lies at the root of the whole matter. The principle upon which it depends is one that was first formulated by Huygens, one of the leading names in the development of the undulatory theory of light. In this diagram (Fig. 3) is represented in section the different parts of the obstacle. C represents the source of sound, B represents the

flame, and A P Q is the screen. If we choose a point P on this screen, so that the whole distance from B to C, reckoned through P, viz. B P C, exceeds the shortest distance B A C by exactly half the wave-length of the sound, then the circular area, whose radius is A P, is the first zone. We take next another point, Q, so the whole distance B Q C exceeds the previous one by half a wave-length. Thus we get the

FIG. 3.



second zone represented by P Q. In like manner, by taking different points in succession such that the last distance taken exceeds the previous one every time by half a wave-length, we may map out the whole of the obstructing screen into a series of zones called Huygens' zones. I have here a material embodiment of that notion, in which the zones are actually cut out of a piece of zinc. It is easy to prove that the effects of the parts of the wave traversing the alternate zones are opposed, that whatever may be the effect of the first zone, A P, the exact opposite will be the effect of P Q, and so on. Thus, if A P and P Q are both allowed to operate, while all beyond Q is cut off, the waves will neutralise one another, and the effect will be immensely less than if A P or P Q operated alone. And that is what you saw just now. When I used the inner aperture only, a comparatively loud sound acted upon the flame. When I added to that inner aperture the additional aperture P Q, the sound disappeared, showing that the effect of the latter was equal and opposite to that of A P, and that the two neutralised each other.

[If A C = a, A B = b, A P = x, wave-length = λ, the value of x for the external radius of the nth zone is

$$x^2 = n\lambda\frac{a+b}{ab},$$

or, if $a = b$,

$$x^2 = \tfrac{1}{2}n\lambda a.$$

With the apertures used above, $x^2 = 49$ for $n = 1$; $x^2 = 100$, for $n = 2$; so that

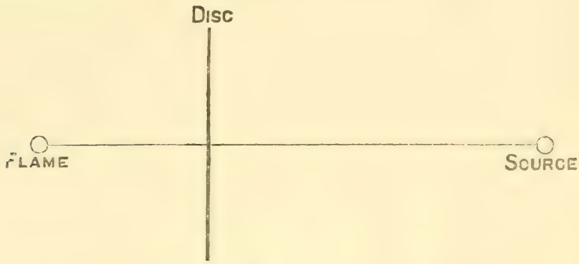
$$\lambda a = 100,$$

the measurements being in centimetres. This gives the suitable distances, when λ is known. In the present case $\lambda = 1 \cdot 2$, $a = 83$.]

Closely connected with this there is another very interesting experiment, which can easily be tried, and which has also an important optical analogy. I mean the experiment of the shadow thrown by a circular disc. If a very small source of light be taken —such a source as would be produced by perforating a thin plate in the shutter of the window of a dark room with a pin and causing the rays of the sun to enter horizontally—and if we interpose in the path of the light a small circular obstacle and then observe the shadow thrown in the rear of that obstacle, a very remarkable peculiarity manifests itself. It is found that in the centre of the shadow of the obstacle, where the darkness might be expected to be greatest, there is, on the contrary, no darkness at all, but a bright spot, a spot as bright as if no obstacle intervened in the course of the light. The history of this subject is curious. The fact was first observed by Delisle in the early part of the eighteenth century, but the observation fell into oblivion. When Fresnel began his important investigations, his memoir on diffraction was communicated to the French Academy and was reported on by the great mathematician Poisson. Poisson was not favourably impressed by Fresnel's theoretical views. Like most mathematicians of the day, he did not take kindly to the wave theory; and in his report on Fresnel's memoir, he made the objection that if the method were applied, as Fresnel had not then done, to investigate what should happen in the shadow of a circular obstacle, it brought out this paradoxical result, that in the centre there would be a bright point. This was regarded as a *reductio ad absurdum* of the theory. All the time, as I have mentioned, the record of Delisle's observations was in existence. The remarks of Poisson were brought to the notice of Fresnel, the experiment was tried, and the bright point was rediscovered, to the gratification of Fresnel and the confirmation of his theoretical views. I don't propose to attempt the optical experiment now, but it can easily be tried in one's own laboratory. A long room or passage must be darkened: a fourpenny bit may be used as the obstacle, strung up by three hairs attached by sealing-wax. When the shadow of the obstacle is received on a piece of ground glass, and examined from behind with a magnifying lens, the bright spot will be seen without much difficulty. But what I propose to show you is the corresponding phenomenon in the case of sound. Fresnel's reasoning is applicable, word for word, to the phenomena we are considering just as much as to that which he, or rather Poisson, had in view. The disc (Fig. 4), which I shall hang up now between the source of sound and the flame, is of glass.

It is about 15 inches in diameter. I believe the flame is flaring now from being in the bright spot. If I make a small motion of the disc I shall move the bright spot and the effect will disappear. I am pushing the disc away now, and the flaring has stopped. The flame

Fig. 4.



is still in the shadow of the disc, but not at the centre. I bring the disc back again, and when the flame comes into the centre it flares again vigorously. That is the phenomenon which was discovered by Delisle and confirmed by Arago and Fresnel, but mathematically it was suggested by Poisson.

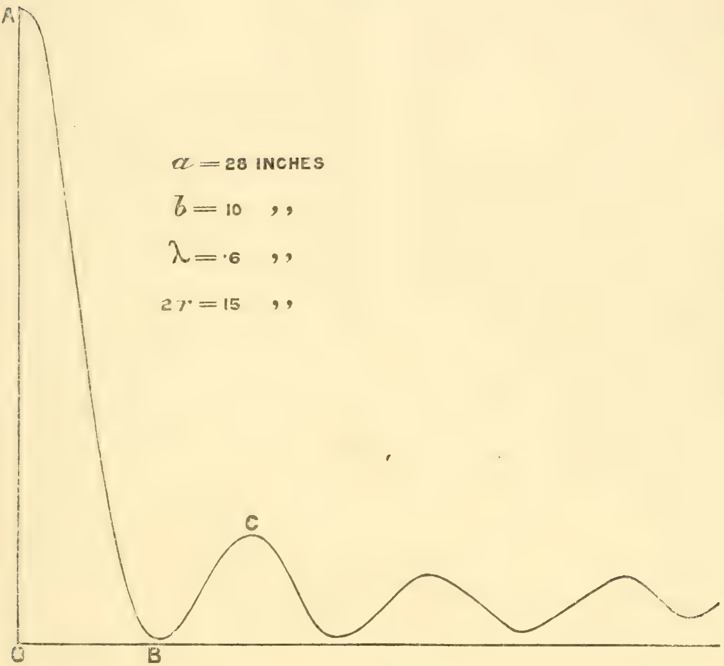
Poisson's calculation related only to the very central point in the axis of the disc. More recently the theory of this experiment has been very thoroughly examined by a German mathematician, Lommel; and I have exhibited here one of the curves given by him embodying the results of his calculations on the subject (Fig. 5).

The abscissæ, measured horizontally, represent distances drawn outwards from the centre of the shadow O; the ordinates measure the intensity of the light at the various points. The maximum intensity O A is at the centre. A little way outwards at B the intensity falls almost, but not quite, to zero. At C there is a revival of intensity, indicating a bright ring; and further out there is a succession of subordinate fluctuations. The curve on the other side of O A would of course be similar. This curve corresponds to the distances and proportions indicated. a is the distance between the source of sound and the disc; b is the distance between the disc and the flame, the place where the intensity is observed. The numbers given are taken from the notes of an experiment which went well. If we can get our flame to the right point of sensitiveness we may succeed in bringing into view not only the central spot, but the revived sound which occurs after you have got away from the central point and have passed through the ring of silence. There is the loud central point. If I push the disc a little we enter the ring of silence B;* a little further, and the flame flares again, being now at C.

* With the data given above the diameter of the silent ring is two-thirds of an inch.

Although we have thus imitated the optical experiment, I must not leave you under the idea that we are working under the same conditions that prevail in optics. You see the diameter of my disc is 15 inches, and the length of my sound-wave is about half an inch.

FIG. 5.



My disc is therefore about 30 wave-lengths in diameter, whereas the diameter of a disc representing 30 wave-lengths of light would be only about $\frac{1}{1000}$ inch. Still the conditions are sufficiently alike to get corresponding effects, and to obtain this bright point in the centre of the shadow conspicuously developed.

I will now make an experiment illustrating still further the principle of Huygens' zones, which I have already roughly sketched. I indicated that the effect of contiguous zones was equal and opposite, so that the effect of each of the odd zones is one thing, and of the even zones the opposite thing. If we can succeed in so preparing a screen as to fit the system of zones, allowing the one set to pass, and at the same time intercepting the other set, then we shall get a great effect at the central point, because we shall have removed those parts which, if they remained, would have neutralised the remaining parts.

Such a system has been cut out of zinc, and is now hanging before you. When the adjustments are correct there will be produced, under the action of that circular grating, an effect much greater than would result if the sound-waves were allowed to pass on without any obstruction. The only point difficult of explanation is as to what happens when the system of zones is complete, and extends to infinity, viz. when there is no obstruction at all. In that case it may be proved that the aggregate effect of all the zones is, in ordinary cases, half the effect that would be produced by any one zone alone, whereas if we succeed in stopping out a number of the alternate zones, we may expect a large multiple of the effect of one zone. The grating is now in the right position, and you see the flame flaring strongly, under the action of the sound-waves transmitted through these alternate zones, the action of the other zones being stopped by the interposition of the zinc. But the interest of the experiment is principally in this, that the flame is flaring *more* than it would do if the grating were removed altogether. There is now, without the grating, a very trivial flaring;* but when the grating is in position again—though a great part of the sound is thereby stopped out—the effect is far more powerful than when no obstruction intervened. The grating acts, in fact, the part of a lens. It concentrates the sound upon the flame, and so produces the intense magnification of effect which we have seen.

[The exterior radius of the nth zone being x, we have, from the formula given above:

$$\frac{1}{a} + \frac{1}{b} = \frac{n\lambda}{x^2};$$

so that if a and b be the distances of the source and image from the grating, the relation required to maintain the focus is as usual,

$$\frac{1}{a} + \frac{1}{b} = \frac{1}{f},$$

where f, the *focal length* is given by:

$$f = \frac{x^2}{n\lambda}.$$

In the actual grating, eight zones (the first, third, fifth, &c.) are occupied by metal. The radius of the first zone, or central circle, is 3 inches, so that $x^2/n = 9$. The focal length is necessarily a function of λ. In the present case $\lambda = \frac{1}{2}$ inch nearly, and therefore $f = 18$ inches. If a and b are the same, each must be made equal to 36 inches.]

[RAYLEIGH.]

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* Under the best conditions the flame is absolutely unaffected.

WEEKLY EVENING MEETING,

Friday, January 27, 1888.

EDWARD WOODS, Esq. M. Inst. C.E. Vice-President, in the Chair.

JOSEPH THOMSON, Esq. F.R.G.S.

GOLD MEDALLIST OF THE ROYAL GEOGRAPHICAL SOCIETY.

The Exploration of Masăi-Land.

THE speaker, in a few introductory remarks, explained that he only proposed to try and conjure up a reflection of a few characteristic features of African scenery and to give his hearers some flavour of travel in savage lands. For this purpose he would draw upon his experiences in Masăi-Land while in command of the Royal Geographical Society's expedition to Victoria Nyanza in 1883–4.*

He commenced his sketches by a description of his first momentous experience—the start from the coast, when, standing on the heights of Rabai, he took his last look around before setting forth inland towards the unknown. Between the coast and the borderland of the Masăi lay 200 dreary, uninhabited miles. To embody the experiences of the expedition over this zone and give his audience some idea of the nature of the country, Mr. Thomson sketched in some detail the routine of a typical day in their progress through this wilderness—the scene in the camp at daybreak, the order of march, the nature of the sterile waste through which they passed; the effects of the broiling sun, the burning sands, and the absence of water on the fatigued porters, resulting in the transformation of their leaders from eager, enthusiastic explorers into harassed and disgusted slave-drivers—all these aspects of the march were sketched, as well as the travellers' delight and relief when, with the declining sun, they arrived at a horrid, liquid mud hole.

At length the neighbourhood of Kilima Njaro was reached, when, with marvellous abruptness, they exchanged glaring sands and desolate wastes for cool shades and leafy labyrinths. The effect upon the travellers was as if they had passed from a purgatory to a paradise.

Through the little African Arcadia of Taveta the speaker now personally conducted his hearers, pointing out the characteristic aspects of its luxuriant vegetation, leading them by shady pathways to bosky glades, or calling here and there at the cosily ensconced beehived-shaped hut of a native. He drew a tempting picture of Taveta at night, when millions of cicadæ piped forth their melodious notes, and myriad fireflies flashed meteor-like athwart the gloom, and

* Mr. Thomson's work, entitled 'Through Masăi-Land,' was published in 1885.

starry glowworms gleamed with mellow light, and turned the dull earth to a second firmament.

After sketching their first glimpse of Kilima Njaro the speaker rapidly conveyed his hearers round the southern and western aspects of this great mountain to the borderland of the Masăi. He told how they crossed the threshold with their hopes at the highest, and how, two days later, they found it necessary to run away and return to Taveta. There the expedition was camped while its commander returned to the coast for additional men and goods, doing on one occasion without a halt nearly seventy miles, without a drop of water or bit of food—an unprecedented feat of African pedestrianism.

On his return to Taveta the expedition once more set forth towards Masăi-land with unabated enthusiasm. The various characteristic incidents of the march were touched upon—such as how they stalked a donkey in mistake for a rhinoceros ; the scattering of the caravan by furious rhinoceroses ; men and donkeys pitched into the air by a mad buffalo bull running amuck through the camp ; a grass fire threatening the expedition with destruction, and other incidents. Helped along by such piquant variations in the daily routine of the march, Masăi-land was once more reached, and they then embarked on a course of romance and adventure sufficient to sate the most ardent of modern knights-errant.

To illustrate the life of an explorer among a people with whom murder is a pastime and robbery an amusement, Mr. Thomson sketched the events of a typical afternoon in camp, in which also he took occasion to touch upon the strange customs of the Masăi. He ended his narrative by indicating the route taken by the expedition to Victoria Nyanza, the exploration of the beautiful plateaux of Masăi-land and the curious meridional trough which divides them, the visit to the snow-clad Kenia, the arrival at Baringo, and subsequent march to Victoria Nyanza, with all the attendant adventures.

A few words sufficed to tell the tale of the return, how the speaker was tossed and nearly killed by a buffalo, and had to be carried back to Baringo on a stretcher ; how his troubles did not end there, for the penalty of drinking poisonous water, eating diseased meat, flour half mixed with grit, and buffalo beef as tough as old boots, had to be paid in the usual way. He was attacked by dysentery, and reached Lake Naivasha at the point of death. Here for two months he lay in a most critical condition, till, despairing of improvement, he concluded that if he was doomed to die he had better do so trying to reach the coast. To his surprise he began to improve, in spite of the terrible jolting in the hammock. Taking a new route they passed through a desert country, where the men suffered greatly from famine. Sixteen months after the departure from the coast the expedition once more found itself among the palm groves of Rabai, with the cares and anxieties of travel lifted from their minds.

WEEKLY EVENING MEETING,

Friday, February 2, 1888.

Edward Woods, Esq. M. Inst. C.E. Vice-President, in the Chair.

Frank Crisp, Esq. LL.B. B.A. V.P. and Treas. Linn. Soc. Sec. R.M.S. *M.R.I.*

Ancient Microscopes.

(Abstract deferred.)

GENERAL MONTHLY MEETING,

Monday, February 6, 1888.

Edward Woods, Esq. M. Inst. C.E. Vice-President, in the Chair.

The Hon. Dudley Campbell,
Mrs. Dewar,
Joseph Emerson Dowson, Esq. M. Inst. C.E.
The Right Hon. Sir Edward Fry, Lord Justice of Appeal,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. P. F. Campbell-Johnston for his present of a Terra Cotta Bust of Sir Humphry Davy.

The Special Thanks of the Members were returned to Mr. James Wimshurst, *M.R.I.* for his present of a large Electrical Influence Machine.

The Managers reported, That at their Meeting held on January 11th last, they appointed Mr. George John Romanes, M.A. LL.D. F.R.S. Fullerian Professor of Physiology for three years.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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WEEKLY EVENING MEETING,

Friday, February 10, 1888.

His Grace the Duke of Northumberland, K.G. D.C.L. LL.D.
President, in the Chair.

William Henry Preece, Esq. F.R.S. *M.R.I.*

Safety Lamps in Collieries.

On a fine summer's day the sun expends an average of one horse-power on every 30 square feet of the earth's surface in this latitude, or 1450 horse-power per acre. This great gift of energy is neither utilised nor stored by man at present, though Nature presents us with some of it in waterfalls and flowing streams. The sun itself has been more generous. Ages upon ages ago it shone with resplendent glory on a grand luxuriant flora of a uniform but flowerless character in a climate warm and damp. England formed part of a tropical jungle or swamp, where grasses, mosses, ferns, and sedges, *coniferæ*, *araucariæ*, *equisetaceæ*, *sigillariæ* grew and flourished, perished and fell *in situ*, to be covered up by the following geological formations and compressed into those grand seams of coal that form now the principal source of England's greatness and wealth. If language be fossil poetry and words condensed history, then coal may be derived from *coelum* or κοιλος (*koilos*), and imply a Heaven-sent gift.

The first authentic account of coal digging dates from the reign of Henry III., when Newcastle sent its first cargoes by sea to London, and thus gave the mineral the name of *sea-coal*. Early records of the output of coal are wanting.

In 1700 it was 2,614,000 tons.
In 1800 it was 10,000,000 tons.
In 1850 it reached 42,000,000 tons.
In 1886 it reached 157,518,482 tons.

A ton of coal occupies very nearly a cubic yard of space. Hence this quantity of coal, if placed in Hyde Park, would form a mountain as high as Snowdon, and if erected as a wall around the coast-line of Great Britain, it would be 6 ft. wide and 30 ft. high. It would form a girdle round the world 3 ft. wide and 12 ft. high. We extract from the earth twenty times more energy than the sun can possibly restore to it by the growth of forest, and we are gradually but surely tending towards the time when this store of power, heat, and light, will be exhausted, when our busy hives of industry must seek other shores, and when England will perhaps become a summer resort for a wealthy Anglo-Saxon race occupying other quarters of the globe.

Coal is not only, like knowledge, power, but, like the precious metals, it is wealth. The average price of coal at the pit's mouth in 1886 was 5*s.* per ton, and thus its total value was 40,000,000*l.*, but when we consider the average price paid for it, we must double, if not treble, this lump sum. The number of collieries open was about 3500, and since it costs 100,000*l.* to open a colliery 2000 ft. deep, when we think of the railways, the canals, the ships, the shops, the marts employed for its transit and its distribution, we can only conclude that the capital embarked in this industry must equal, if not exceed, the National Debt.

The hewing and raising of coal is unquestionably a dangerous occupation. "The price of coal is pit-men's lives," said an old collier to George Stephenson. 520,000 persons are employed in our collieries, and the output per person is 308 tons. 953 persons met their deaths in 1886; but it is satisfactory to know that a miner's life is more than twice as safe now as it was thirty years ago.

The number of persons employed per death was—

In 1856	242
In 1886	545

The number of tons raised per life lost was—

In 1856	64,700
In 1886	188,800

Most people, if asked what was the principal cause of this loss of nearly 1000 lives, would answer explosion of fire-damp, but this would not be true. The principal cause of accident is falls of roof and sides. In 1886 there were from—

Falls of roof and sides	461 deaths
Explosions	129 „
Trams and tubs	81 „
Miscellaneous	282 „

Thus falls of roof form 41 per cent. of the whole, and explosions 23 per cent.

Explosions attract immense attention, from their publicity and their appalling suddenness and magnitude. It is dreadful to take up a morning paper and read of 268 fellow creatures engulfed, as at Abercarne in 1878, or 164 at Seaham in 1880; but the daily and steady loss of life by ones and by twos fails to get chronicled, and passes unheeded by with the very great majority of the 20,000 violent deaths that occur in these islands every year.

In order that you may see how coal is won, and hewed, and raised, how dangers are incurred and surmounted, I will take you into a coal-mine—one of the Cannock Chase collieries of Staffordshire—by aid of a beautiful series of photographs, taken by means of magnesium light by my friend, Mr. Arthur Sopwith, the eminent manager of those mines.

1. We commence with a map of Great Britain, showing in shaded lines the distribution of the various coal-fields.

2. A view on the surface of the pit's mouth, showing the engine-house, the pulleys, frames and ropes, with their motion up and down their respective shafts, the banking shed for tipping the raised coal into railway waggons and carts, the waggons laden with coal, the trucks laden with wood for "cogging," and timber for treeing, supporting, and strengthening. This particular pit raises about 1000 tons in eight hours, and it employs from 500 to 600 men.

3. This is the top of the shaft, with the cage in position ready to descend with an empty tub. Every pit has two shafts, the up-cast and the down-cast, for working and ventilation. They vary from 10 feet to 20 feet in diameter. This particular pit is 360 feet deep, but there are several in England over 2000 feet. The rate of descent is 18 feet per second, or about 20 miles an hour.

4. This is the bottom of the shaft; the tub, laden with coal, just brought from the working face, the man in the act of running it on to the cage, and his hand in the act of signalling to the surface.

5. An overman's cabin hewn out of the coal, the underviewer making his report of the condition of the mine after his morning inspection.

6. The engine-plane or level, which is 2000 yards long. A hewer, pike in hand, meets the underviewer with his "Clanny," and learns the state of the mine. The truck is worked by an endless rope, it has a double way, the laden tubs drawn towards the shaft, and the empty tubs towards the workings. It will be seen that the roof is strengthened by iron bars instead of the timbers generally used.

7. Clipping a tube to the rope by means of a shackle and coupling. The ordinary cross timbers or bars are here shown supporting the roof.

8. A level branch off the engine-plane. Horses or ponies now take up the work and draw the empty tub through a "gob" road—a road through the whole working—to the working face or long wall. Though a horse in the pit does scarcely one-half of the work of a horse on the surface, it lives as long. There are horses that have never seen daylight for 16 years.

9. A road near the face; men resting from their work—taking their lunch or "jack bit."

10. The end of the road, the tub taken off the rails and dragged to the face.

11 and 12. The working face. Undercutting; taking away the hard under-clay preparatory to blowing or wedging down the superincumbent coal.

13. Punching a hole for blasting with powder. The mode of supporting a "gob" road by cogging is very well shown in this slide.

14. A way end, showing the result of a blast.

15. "Bannocking" or holing the top; the reverse to undercutting, the charge being put in at the bottom and the explosion acting upwards.

16. Drilling a hole for a lime cartridge. The anomaly of using gunpowder and safety lamps in the same place is destroyed by the use of caustic lime, which forces out the coal by the expansive action of water on the lime.

17. The act of watering the lime by pumping.

18. The result, the fallen coal.

19. Building a cog-wall—a strong boundary to the "goaf" or gob, which consists of the refuse of the old working and the subsident roof.

20. Rock "ripping," clearing a roof which has subsided and reducing the height of the way, so as to leave room for the horses to go through the old working. This is the most dangerous operation connected with coal mining.

21. Setting trees or upright timbers to support the roof. Each tree or stanchion has a cap or lid, and they are placed 6 feet apart.

22. Drawing timber by means of a chain for use again, so as to allow the roof to fall or subside uniformly, and not to break up in pieces.

23. Examining a waste for gas. This is done two or three times a day by special firemen with the ordinary safety lamp.

24. Tapping old working for water; a source of great trouble in collieries.

25. Trying the roof.

26. A surveying party. A fault in the seam is shown in this slide.

27. The furnace used for ventilation. This has now been abandoned for more perfect and less wasteful mechanical contrivances.

I have shown you most of the operations connected with the winning and working of coal; some of the risks the miner incurs; some of the troubles arising from gas, water, and falling roofs; and one of the modes of producing ventilation—the chief prevention of accident. What our mines could have been in days gone by it is impossible to conceive; now mechanical appliances are so admirable that many mines are as perfectly ventilated as our homes. The temperature below ground is so uniform—50° F. at 50 feet, and rising 1° for each 55 feet—the formations are so dry that I have actually heard it proposed to establish a sanatorium for consumptive patients underground, lighted by electricity, and supplied with every luxury.

All mines are required to have two shafts—one the *intake* or down-cast, the other the *return* or up-cast, and the workings are so interlaced with ways and roads, doors are inserted here and there to direct the current, anemometers are used to measure its rate of flow, that any ordinary inroad of gas is swiftly swept away. Fire-damp, or pit

gas, is marsh gas ($C H_4$). It oozes out gently from the exposed seam, or sometimes it bursts out through some fissure with great force, forming what are called "blowers." If it be mixed with air, in the proportion of from 5 to 9 per cent. of gas, it becomes highly explosive, and is the prime cause of those fearful disasters that have made coal mining so terrible.

In gasless mines candles always have been and still are used, but in early days, in foul places, men had frequently to work in the dark, or to be content with the feeble illumination of the phosphorescence of decaying fish. It is remarkable how the eye adapts itself to feeble light, and in the Cimmerian darkness of a coal-mine even phosphorescence has a useful illuminating effect. In many places they used the *steel mill*, a disc of steel rotated rapidly against flint, giving light by the shower of sparks thrown down. It is most remarkable that no scientific thought was devoted to this subject until 1815. In 1813 Dr. Clanny, of Newcastle, had devised a very poor lamp, the air to support which was driven by bellows through water; but in 1815 Sir Humphry Davy devoted his powerful mind and skilful hands to solve the question, and speedily invented in this very Institution his immortal safety lamp. It is a remarkable coincidence that in the same month another powerful but untutored mind, by strong observant powers and pure mechanical reasoning, had arrived at very nearly the same result; and even to this day the affectionate remembrance and name of the eminent Northumbrian brakesman, George Stephenson, is maintained by the use of the "Geordie," in his old home, the Killingworth Colliery.

Davy's classical paper was read before the Royal Society, on November 11, 1815, and was entitled "On the Fire Damp of Coal Mines; and on Methods of Lighting the Mine so as to Prevent Explosion." He showed that flame would not pass through small tubes and apertures, and how in comparatively still air, however charged with gas, a wire gauze surrounding the flame so reduced the temperature by radiation that explosion was impossible. Ingress of air and egress of products of combustion are resisted. He showed that a flame so protected gave immediate intimation of the presence of gas by burning dimly, and by becoming capped with a blue flame, or *aureole*, that though the wire gauze became redhot, it still radiated away the heat sufficiently to prevent its reaching the temperature point of explosion. He also pointed out, which has been strangely neglected, until enforced recently by the Royal Commissioners on Accidents in Mines, that it failed to act in a current of air, but that this effect of currents could be diminished by shielding or protecting the inlet of air.

The main principle discovered by Davy is the basis of all safety lamps burning oil or spirit, but several departures in form have been proposed at different times. The "Geordie" is in reality a Davy lamp with a glass shield. The "Jack" lamp is a Davy in a tin can. The "Clanny" is a Davy lamp with the flame portion surrounded

by a thick glass cylinder to increase the emission of light, and now with a bonnet or shield to protect it from currents of air. The "Mueseler," or Belgian type, is a "Clanny" with a central metallic cone acting as a funnel to increase the draught, and therefore the light. The "Marsaut," or the French type, is a bonneted or protected "Clanny," with two and three gauzes added to increase the security. The number of lamps is legion. A new intake here, a fresh egress there, a new direction to the currents of air feeding the flame, a change in the form or character of the wick, a lock, a different form of pricker, a shut-off or extinguisher (automatic or manual), is quite sufficient to justify a new patent and a new name. The Royal Commission experimented upon 250 different kinds of lamps.

Since the commencement of the year a new Act has come into force rendering it unlawful to use the plain "Davy," "Stephenson," or "Clanny" lamp in mines where safety lamps are necessary. They must be shielded, and great numbers are being converted into bonneted "Marsauts" and "Mueselers." They are also being constructed to burn the best vegetable oil (rape or colza), to which one-third mineral oil is added, as recommended by the Royal Commission, to enhance the light-giving power of the lamp. These converted lamps have baffled all attempts to explode them in currents of explosive gases of very high velocity, and under the most rigid tests.

There are several objections to these oil lamps. They go out, or are put out when danger exists; they render that most important and essential duty, examination of the roof, difficult and insufficient. If they become extinguished by accident or design, much time is necessarily wasted in getting them relighted, an operation that must be done at or near the shaft, perhaps two miles away from the working face, and which therefore reduces the output of coal and the earnings of the collier. The light they give, even at the best, is very small—about one-third to half of a candle.

There are several dangers present. The Royal Commission said:—"The source of light within the lamp should be unable under any circumstance, at all likely to occur in working coal to cause the ignition of an inflammable mixture of fire-damp and air, even when this is passing at a high velocity." But glasses break by water, heat, falling coal, and accident. Joints get loose and bad even when protected by asbestos washers.

They prove a constant temptation to the thoughtless and callous to get a light. The perfect light does not exist at present, but while these mechanical lamps are very excellent in their way, electricity seems likely to step in and supply their deficiencies and fill a decided want. An electric lamp can be made to give any desired amount of light, but anything between half a candle and a candle seems easily attainable, and such a light can be maintained steady and bright in explosive gases and in strong currents of air. They are simple in construction, easy of inspection; they are not likely

to be extinguished in handling like the Mueseler; but they do not act as detector of the presence of gas, and they might explode gas if their protecting glass shield were accidentally broken.

It is remarkable that Davy himself in 1815 experimented with an arc lamp in a closed vessel of glass, but it was not until 1865 that a practical lamp was proposed for collieries. Dumas and Benoit used a small Geissler tube illuminated by sparks from an induction coil and excited by a primary battery; but it was heavy—it was a kind of knapsack to be carried on the back, and it met with no success.

Mr. Swan has been more successful. The great success of the glow lamp, in the introduction of which he has played so prominent a part, and the perfection of the secondary battery have enabled him to produce a lamp that compares in weight and size and light with the best mechanical lamp. Eight hundred of these lamps are in constant use in the National Colliery in the Rhondda Valley in South Wales, and 1600 more are on order for the Risca and Abercarne Collieries belonging to Messrs. Watts, Ward, and Co. They are giving great satisfaction. A collier told me that the electric light was "as good as the moon." The lamp batteries are charged a great number at a time in blocks at the pit bank.

Mr. Pitkin has also been very successful in making light portable lamps, and they have been practically used in Cannock Chase and at the Tyldesley Collieries in Lancashire.

The "Sun" lamp is another very promising form, worked by secondary battery. It is light—3 lb. 12 oz.; it gives $1\frac{1}{4}$ candle, and this is maintained for ten hours. Secondary cells usually require about twelve hours' charge to emit a ten hour discharge, but this cell will give a ten-hour discharge with four hours' charge. It is fitted with a safety appliance—a plan patented by Mr. Sennett in 1882—by which the lamp is automatically switched out the instant that the outer protecting glass is broken or cracked, so that contact between the hot filament of the lamp and the explosive gas is prevented. This safety appliance has been tested by Mr. Rhodes at the Aldwarcke Colliery in actual explosive mixtures.

Many efforts have been made to introduce primary batteries for the same purpose, but up to the present moment I have seen only one—the Schanschieff—which, in efficiency, lightness, and economy comes up to the requirements of a safety lamp. A primary has this advantage over a secondary battery—that it is charged at once by an operation as simple as that of trimming a Davy. To trim a Davy means thorough cleaning, inserting a fresh wick, pouring in new oil. A primary battery means pouring out old, and pouring in fresh solution.

Occasionally zincs require renewal, but all these operations are within the intelligence of the collier, and the lamps need not go to the surface. The simplicity of the operation has distinct merits of its own that compensate for the extra cost of the materials used.

The chief charm of the electric lamp is not the greater light that

it gives, but the power it gives the viewer to examine the roofs of the ways and workings; it is so extremely portable and handy; it can be put anywhere; the battery can be placed on the ground or suspended by a spike to a tree, and the lamp can be fixed in the cap or around the waist; it can be put anywhere and used anyhow.

Mr. Sopwith, at Cannock Chase, has been taking heavy batteries to the working face, and using bright 5-candle lamps in reflectors, so as to illuminate the working face with a light which in that region is comparable to daylight. The men are charmed with it.

The chief defects of the electric lamps are their fragility, the liability of the carbon filament to break, the weight of the battery, and the absence of gas detection; and these defects have been very much enhanced by imperfect construction and injudicious details. Electricians have not spent sufficient time in the lower regions, and practical colliers have no time to go to the laboratory. When the two professions are properly amalgamated we shall very likely obtain the true safety lamp.

The electric lamp is not a fire-damp detector, but an electrical appliance for this purpose is easily added. Liveing and Swan have done this; but it is doubtful whether a more efficient detector than the Davy exists, and whether an apparatus that is so thoroughly understood, and so thoroughly practical, will be superseded for a purpose for which it seems so eminently adapted.

I cannot conclude, especially in this place, with more pregnant words than Davy's own:—"The gratification of the love of knowledge is delightful to every refined mind; but a much higher motive is offered in indulging in it when that knowledge is felt to be practical power, and when that power may be applied to lessen the miseries or increase the comforts of our fellow creatures."

[W. H. P.]

WEEKLY EVENING MEETING,

Friday, February 17, 1888.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Sir Henry Doulton, *M.R.I.*

Some Developments of English Pottery during the last fifty years.

The opportunity for the efforts of the potters of each succeeding age lies in the fact that the manufacture is based on conditions which are constantly varying, and sometimes even startling in their unexpected results. The material with which the potter deals is as varied as are the localities from whence it is taken, and as infinitely diversified as are the workers' tastes and ingenuity. And further, the final and essential process, the subjection to fire, "which tries every man's work of what sort it is," and especially tries the potter's work, is hidden from view, and under this condition the last stages of the work may be heightened in effect or hopelessly marred.

The progress of the art of pottery affords many striking instances of the laws of growth, perfection, and decay. As schools of philosophy, poetry, and painting are subject to rise, culmination, decline and fall, so is the potter's art. It is, however, a striking fact, that, with hardly any exception, only those potters have been able to maintain a long-lived career who have relied for their staple manufacture on utilitarian rather than decorative wares. A proportion of the useful seems to be an essential condition of any degree of permanence.

A school of artistic pottery is short-lived, firstly, because it is dependent upon individual taste and culture, and, secondly, because it is not remunerative. Wedgwood, Worcester, and Minton have undoubtedly maintained their continuous production through so long a period by careful attention to the requirements of domestic as well as ornamental pottery.

I propose, in the limited time at my disposal this evening, to indicate the progress of the last fifty years in both useful and artistic pottery, and the happy blending of both qualities in some directions.

Professor Church, in his masterly work on English pottery, says, "About the year 1790, the careful, elegant, and rich wares which had held their own for nearly half a century, were gradually displaced by more gorgeous productions, covered with gilding, and possessing even less freedom and spontaneity than the works of Chelsea and Etruria,

in fact, vulgar when not merely feeble." The decadence which then set in continued until the commencement of the new renaissance with the reign of Her Majesty.

From its purity and durability, pottery, embracing the finest porcelain and the roughest earthenware, has been a useful adjunct—I might almost say an absolute necessity—for the sanitarian, the chemist, the architect, the agriculturist, and the electrician.

The rapid advance of chemical and other scientific discoveries, and the application to manufactures of scientific processes, has also led to a vastly increased demand for pottery suited to such operations; while the gigantic development of telephone and telegraph has had its proportionate effect on pottery production. The impetus given to metallurgy has brought about a greatly extended use of crucibles, and the rapid advance of sanitary science has given a distinct impulse to pottery manufacture. Indeed, pottery has in no small degree conduced to the satisfactory solution of the great sanitary problems of the present age. The application of terra-cotta, too, has led to the most successful results.

The same advances in scientific research which have multiplied the demand for pottery wares have had an equal influence in improving their quality and efficiency, as well as reducing the cost of their production.

In 1846, anticipating an extensive use of stoneware for street and house drainage, I began a special factory for its production, and since that time the manufacture has increased till it may now be considered a great national industry.

The introduction of baths, sinks, lavatories, glazed bricks, ventilating and syphon traps, irrigation pipes, and many other inventions have in late years provided the means of greatly reducing the death-rate of large communities, and it is not too much to say that sanitary science has advanced *pari passu* with the use of pottery.

The increasing demand for chemical vessels of stoneware capable of resisting acids may be regarded as giving the first important impulse to the Lambeth Potteries. At the present time, care and experience, together with better machinery for the preparation of the material, makes it practicable to produce vessels of four or five hundred gallons capacity.

Under the category of things both useful and beautiful in pottery, are earthenware tiles. In 1840, R. Prosser, of Birmingham, obtained a patent for the manufacture of buttons by reducing the material of porcelain to a dry powder, and subjecting it to strong pressure between steel dies. Prosser disposed of part of his interest in this patent to Minton, who then made some very beautiful buttons and studs. In 1841 Blashfield conceived that this process might be extended to the manufacture of small tiles and tesseræ. In 1843, the process of manufacture was exhibited by Prosser and Blashfield at a meeting of the Royal Society, when the late Prince Consort took great interest in the making of tesseræ, and desired an account of the whole subject to be sent him.

To those best able to appreciate the obstacles to be overcome, the results achieved in this direction by Minton, and later on by Maw, stand out as triumphs of patient technical research.

I cannot here refrain from rendering my tribute of admiration to the beautiful lustre pottery of De Morgan, which has had its principal application to tiles. Of all the adaptations of Persian and Hispano-Moresque art, his has been the most successful. Maw & Co. have, however, lately almost rivalled in their ordinary productions the exquisite work of this school.

Nor must we fail to notice the constructive faïence or glazed terra-cotta so successfully produced during the last few years by Messrs. Minton, Wilcock, Cliff, and Doulton, which has lately become a recognised method of embellishment for many of our large hotels.

Of late years the gradual introduction of fireclay into open fire-places in the shape of slabs for back and sides has directed attention to the desirability of its introduction in England in the form of closed stoves.

Decorative Pottery.—Looking back over the last half-century, it is apparent that progress rather than discovery has marked the age so far as pottery is concerned. The 18th century is filled with the names of pioneers in the potter's art in this country, whose patient research has enabled their followers to improve and perfect the results. But it is difficult to recall during the Victorian age many discoverers in plastic manufacture which can be named on the same lines as those of Elers and Ashbury, who introduced ground flint; Sadler of Liverpool, and Dr. Watt of Worcester, who introduced pottery printing; Cooksworthy, kaolin; and Wedgwood, jasper ware; and other important discoverers.

The introduction of lead-glaze and of the process of transfer-printing had completely displaced the demand for salt-glazed stone-ware, which virtually ceased at the end of the last century.

The discovery by Joseph Spode of opaque china was followed by the perfecting of parian. This success was greatly due to Copeland, but there is little doubt that Minton's experiments in the same direction were simultaneous. During the past fifty years there has been continuous development of Messrs. Minton's productions, and their name, together with those of Worcester and Wedgwood, occupy a position of honour among potters in later times.

The masterly executions of Bolt in enamel painting on dark-blue, or "Limoges Worcester," together with the later introduction by Mr. Binns of ivory porcelain at the Royal Porcelain Works, are achievements of which the present era may justly feel proud.

Salt-glazed Stoneware.—In examining the various English wares of early date, stoneware arrests attention as a great advance on all that has gone before. That advance was due, not so much to any patient insight into quality or admixture of material, as to a distinctly novel principle of manufacture, viz. the glazing of ware by vaporous flux while approaching the vitrifying point, common salt being thrown

into the kiln when at full white heat. The decomposed fumes of soda combine with the minute particles of silica in the surface of the ware, and form a thin glassy coating of intense hardness. Experience soon showed the discoverers that the full qualities of the glazing by this method could not be reached except as the ware approached the vitrifying point, so that those means adopted to give brilliancy to the glaze also of necessity produced excellence of body and strength and durability.

Though talented decorators were certainly employed on Delft ware in Lambeth at the early part of the present century, no attempt was made to apply this talent and experience to the embellishment of the salt-glazed ware, which was allowed to sink into a manufacture devoid of all pretence to beauty. Such was the condition of the stoneware manufacture at this time, and such it continued until 1867, when the first efforts were made at Lambeth.

It is somewhat remarkable that the latest introduction of the last half century should consist of an art-ware which, instead of gathering up all the resources and experience of the past, sprang, as it were, from an altogether independent stock, and though in some respects apparently a revival of old traditions, and rising up from the selfsame spot as the Delft potteries, the Doulton stoneware received no inspiration from its predecessors. It has been successful in starting out a path for itself both original and progressive.

In 1868 the idea was conceived of attempting to raise salt-glazed stoneware into an art material. The first attempts at decoration were confined to form and relief with simple coloured bands and runners. To this, "sgraffito" or incised outline filled in with blue, was soon added. A small collection of vases and jugs was exhibited at the International Exhibition of Paris, 1867, and from this time till 1871 no further progress was made. Early in 1871 it was determined to make every effort at Lambeth Pottery to originate examples of decorated stoneware. The result distinctly aimed at was successfully attained, and the greater portion were eagerly purchased for the museums throughout Europe. It is satisfactory to know that the strenuous efforts made during the last fifteen years to maintain the unique and original characteristics of the ware have been appreciated.

The later months of 1873 also brought about the introduction of an entirely new branch of works, viz. Lambeth Faïence, a ware painted under the glaze on a soft biscuit of warm tone and glazed with a lead glaze.

Each International Exhibition called forth renewed effort, and that of Paris in 1878 was still more varied and important. In the following year another new ware was introduced called "Impasto," the decoration being executed on the *soft* clay by means of coloured slips. In the year 1880 the so-called "Silicon" ware was introduced. It consists of a vitrified stoneware impregnated with metallic oxides throughout its mass and coated with a "smear" or semi-glaze.

It is much to be feared that the taste and intelligence of purchasers has not advanced concurrently with the production of really beautiful and artistic works. There is with the public of the present time a morbid craving after novelties, irrespective of their intrinsic excellence, and this craving leaves neither designer nor manufacturer time to develop the full capabilities of his productions before the passing day of public appreciation has gone by.

I might here refer to two original workers whose early training was accomplished at the Lambeth School of Art, viz. Miss Barlow and Mr. Tinworth. Mr. Tinworth undoubtedly owes the recognition and early development of his powers to this School, and opportunities of more advanced study to the Royal Academy. (Lantern views were here shown of some of Mr. Tinworth's works.)

The last fifty years has seen development rather than initiative in pottery treatments, and science even more than art will probably have the greatest influence on this manufacture in the immediate future. I would fain indulge the hope that a large proportion of the works of this period, surviving the ravages of time, will testify in the distant future to the combined beauty and utility of the production of the English potter in the Victorian age.

[H. D.]

(The lecture was illustrated by a series of specimens, and in the library was exhibited a collection of the productions of Doulton, Minton, Worcester, Maw, and De Morgan, including some examples of Messrs. Doulton's Burslem ware.)

WEEKLY EVENING MEETING,

Friday, February 24, 1888.

Edward Woods, Esq. M. Inst. C.E. Vice-President, in the Chair.

The Very Rev. G. Granville Bradley, D.D.
Dean of Westminster.

Westminster Abbey.

The lecturer, who spoke merely from a few notes, began by dwelling almost in despair on the extraordinary range and multifarious aspects of his subject. He gave a sketch of three or four lines of thought to which he or others speaking on such a subject might have confined themselves. He might have found a rich field of interest in tracing the history of the mere fabric, from the day when the Norman builders of Edward the Confessor reared the structure which covered nearly or quite the same ground as that on which its more stately successor stands to-day. He might, avoiding technical and architectural details, have described the work of Henry III., and its slow and gradual completion, till at last, after five centuries of active work and of long pauses, its western towers stood up new and glistening in Hogarth's picture of St. James's Park now on view. Or he, or one more competent, might have called their attention to the Abbey as a museum of priceless value, from the merely artistic side of its contents, including as they do a continuous series of English and foreign sculpture from the thirteenth century down to yesterday. Or, again, he might have tried to put before them something of the inner life and outward history of the great Benedictine monastery of which this historic church formed but a part. How few of the thousands of visitors to the Abbey thought of the generations of monks who for 500 years paced those cloisters, slept in that dormitory, and sang and knelt in that choir, whose abbots were the treasurers, or counsellors, or secretaries, or ambassadors, or gaolers of kings, whose domains and manors comprised Hyde Park and St. James's, much or all of Kensington Gardens, and such districts as Chelsea, Paddington, Belgravia, and Covent Garden. Or, fourthly, he might have put before them, by the aid of photography, illustrations of the tombs, and spoken of the historic memories which they awake, alike in themselves and in their often touching juxtaposition.

But he would turn from these and other inviting vistas of thought, and content himself with putting before them a few hints as to the various influences whose combined action had given the Abbey such a hold on the affections of all who speak our language, and which

gave a hundred-fold force in the present day to the striking words of Edward IV., who in a letter to the Pope, written over four centuries ago, spoke of it as dear to the *orbis Anglicanus*—the "whole English world."

What were then, what are now, the claims to so unique a position of a church whose legal title is "*the Collegiate Church of St. Peter, Westminster*"? (1) It was no doubt a great *monastic* church, as its very name of *Abbey* implied. Its very legends, such as that of the consecration of the first rude church by St. Peter himself, were closely intertwined with its real history, and had aided its abbots in their pertinacious and successful efforts to assert their entire independence of the English Episcopate. But had this been all, its interest might have grown pale when its days as an abbey church came to an end under Henry VIII., when for a short time it became what Shakespeare calls it—a "Cathedral Church"—and reappeared with its present constitution. It owed its singular position to exceptional causes. For (2) it was the great monument raised by the last of the English Kings who were heirs of Alfred; it was reared by Edward the Confessor as his own burial-place. And as such the new and foreign dynasty of the Conqueror claimed a share in it as his heirs, assumed their crowns one after another by his graveside, till at last the fusion between Norman and Englishman was marked by the erection of the most important part of the new church by Henry III., who chose his own place of sepulture by the side of the Shrine in which he placed the body of the sainted King. Round that shrine, with some interesting exceptions, slept his sons and successors, and it became more and more what Edward III. expressly calls it, the colossal "Royal Chapel" of the Kings of England, and its history became in every generation more and more intertwined with the history of England as shadowed forth in those Kings—in their accessions, their marriages, their wars, and their deaths. (3) To the people of England also it had become dear—first, as containing the relics of the native King to whose reign they looked back as the golden age of the liberties of England; and later on as the scene not only of great religious ceremonies, but of great pageants held in memory of national triumphs. There, too, in its splendid Chapter-house was the meeting place of the Commons of England, of the Parliament that was to become the mother of Parliaments. And (4) from the awakening time of the Reformation its influence grew and widened. There was not merely the splendid addition made to it on the very eve of that epoch by the chapel of Henry VII., but there was the recognition of other forms of greatness than that of kings and warriors and statesmen and abbots and ecclesiastics, that dated from the erection of the monument to Chaucer and the burial of Spenser. The tide of interest spread and deepened with every generation, till its crowd of monuments touched the memories and affections of Englishmen, Scotsmen, Welsh, and Irish—alike of citizens of the American Republic, of members of our own colonial

empire, and of natives of India—alike of Churchmen and of Nonconformists. The very "jumble and chaos" of monuments of which visitors sometimes spoke had its interest. In the nave, in which all was comparatively modern, Pitt, with outstretched arm, looked over a Jacobite Dean's grave on his right, and his rival Fox's monument on his left. Before him were the gravestones of an Irish Archbishop and a Nonconformist missionary, of sailors, soldiers, engineers, and architects, of Newton, and of Darwin. Far to the east lay in the same vault the first of our Welsh and the first of our Scottish Kings. By a fortuitous juxtaposition Darnley's effigy knelt with its face fixed on the tomb of his wife, Mary Queen of Scots. Behind him knelt his brother, gazing with folded hands on the vault that holds the ashes of his daughter Arabella Stuart. So again, by an ill-considered removal, the tablet of Major Creed, who fell at Blenheim, had been taken from the spot where his mother had placed it, next to the memorial of two gallant officers who faced death in Lord Sandwich's flag-ship, and by which he had so often stood to read their inspiring story.

The lecturer said a few words of the periods of danger as well as of growth through which the actual fabric had passed. It was at this moment once more in peril. He had entire faith that a monument of national history which stood alone in the world would not be suffered to decay and fall.

WEEKLY EVENING MEETING,

Friday, March 2, 1888.

Edward Woods, Esq. M. Inst. C.E. Vice-President, in the Chair.

C. Meymott Tidy, Esq. M.B. M.S. M.R.I.
(Professor of Chemistry and of Forensic Medicine at the London Hospital, &c.).

Poisons and Poisoning.

Toxicology is the science of "poisons and poisoning." How comes "toxicology" to mean "the science of poisons"? The Greek word τόξον (derived perhaps from τυγχάνω) signified primarily that specially oriental weapon which we call "a bow." In the very earliest authors, however, it included within its meaning "the arrow shot from the bow."

In the first century A.D. in the reign of Nero (a poisoner and a cremationist), Dioscorides, a Greek writer on Materia Medica, uses the expression τὸ τοξικόν to signify "the poison *for smearing arrows with*." Thus by giving an enlarged sense to the word—for words ever strive to keep pace, if possible, with a scientific progress—we get our modern and significant expression "Toxicology," the science of poisons and poisoning.

And there, in that little piece of philology (τόξον and τοξικόν—a bow and a poison), you have not only the derivation of the word, but the early history of my subject.

A certain grim historical interest gathers around the story of poisons and poisoning. It is a history worth studying, for poisons have played their part in history.

The "subtil serpent" taught men the power of a poisoned fang. History presents poisoning in its *first* aspect in a far less repulsive form than it has assumed in latter days—(for a world may grow wiser and wickeder withal). Poison was in the first instance a simple instrument of "open warfare." For this purpose our savage ancestors tipped their arrows with poison in order that they might inflict certain death on a hostile foe. It can scarcely be questioned that the poison of the snake was the first material employed for this object. The use of vegetable extracts (such as *curarine*, the active principle of which is strychnine, and is employed at the present time by certain uncivilised communities) belongs to a later period.

And so the first use of poison was for an "open fight." It was reserved for later times to mix the cup of kinship with a treacherous diabolical venom!

"An open fight!" Is the suggestion (think you) too wild, supposing "war chemists" with their powders, their gun-cotton, and their explosives never to have been invented, that nations would have turned for their "*instrumenta belli*" to toxicologists and their poisons. I claim, however (notwithstanding that in this we missed our chance), no more for my subject than its due, if I attempt to localise the very cradle-room of science as the laboratory of the toxicological worker.

Besides snake-poison, the use of animal fluids, either alone or mixed with snake-poison, with which to charge arrows, is pre-historic. Thus in old Greek legend we read how Hercules dipped his arrows in the gall of the Lernæan Hydra to render the wounds they inflicted incurable and mortal, and how at last Hercules himself was poisoned by his wife's present, the tunic of the Centaur Nessus stained with his poisonous blood, which she vainly hoped might restore her husband's affection, but which only procured for him the frightful agonies and tortures of which he died.

The use of putrid blood as a poisonous agent and the admixture of the snake-poison with blood constitutes a curious history, when regarded in connection with our present views on septicæmia. The toxic activity of putrid animal fluids seems to have been recognised in very early times. And I suppose these early observations on the effects of putrid blood explain the view almost universally adopted, that blood itself was a poison. Thus the deaths of Psammenitus, king of Egypt (as recorded by Herodotus), and of Themistocles (as recorded by Plutarch), were said to have been effected by the administration of bullock's blood. Even Blumenbach, so lately as the middle of the last century, persuaded one of his class (by way of settling the question) to drink seven ounces of warm bullock's blood. The young man (good as were his intentions) did not die a martyr to science.

The history of poisons and poisoning, the contents of the first chapter of which I have thus briefly indicated (viz. the toxicity of the snake-poison and of blood), down to the final chapter, which commences with the properties and reactions of arsenic, forms a tempting subject for my lecture to-night. The histories of Circe and Medea—of Livia Drusilla and Locusta—of Tiberius and Nero—of the Borgias—of Hieronyma Spara, Tofana, Catherine de Medicis, St. Croix, and a host of other worthies, have proved charming topics for the marvellous-monger. And it would not have been an unworthy subject to bare the truths underlying the stories of generations of story-tellers, obscured as they have become by the demand of ignorant sensationalism and the terrors of a mean superstition. But this is not the subject I have proposed to myself for the discourse to-night.

"What is a Poison?"

Two difficulties present themselves in answering this question:—

1. *The law has not defined a poison, notwithstanding that the law at times demands of science the definition of a poison.*

I know a case, for example, where a prisoner, indicted for the administration of a poison, escaped, because the scientific witness declined to say that the drug administered by the prisoner was a poison.

2. *The popular definition of a poison is far from being a sound, much less a scientific definition.* Generally speaking, it comes to this, that "A poison is a drug that kills rapidly when administered in a small quantity."

The phrase "a small quantity" as regards weight, and the word "rapidly" as regards time, are as indefinite as the classical piece of chalk as regards size.

I define a poison as—

"Any substance which otherwise than by the agency of heat or electricity is capable of destroying life either by chemical action on the tissues of the living body, or by physiological action after absorption into the living system."

(A) It will be convenient to consider first, *What a poison is not.*

It is not an agent that destroys life by physical influences, such as heat and electricity.

It is not an agent that destroys life by any purely mechanical act (e. g. pins are not poison, although fairly included in the phrase "destructive things").

It is not an agent that destroys life by the mere blocking out of that which is necessary to maintain life (i. e. the action of a substance to be a poison must be more than mechanical).

This latter point requires further consideration:—

Both nitrogen and carbonic acid destroy life as certainly as they extinguish a burning taper. Yet nitrogen is not a poison, whilst carbonic acid is.

Nitrogen simply destroys life by blocking out oxygen. Given the presence of 20 per cent. of oxygen, the 80 per cent. of nitrogen possesses no toxic effect.

The carbonic acid, on the contrary, is specifically toxic. The admixture of 20 per cent. or of 80 per cent. of oxygen does not materially alter the case. Oxygen or no oxygen, CO_2 is a poison.

(B) Consider next, *What a poison is.*

It is an agent capable of destroying life.

The use of the phrase *deadly* poison, is surplusage. If a body be a poison, it is deadly; if it be not deadly, it is not a poison.

My definition limits the mechanism whereby the toxic effect is induced, to *chemical* and *physiological* actions. I am conscious that this definition suggests classification. Certain is it, that Nature hates classification as truly as she declines definitions.

Let us trace some of these mechanisms of toxic activity. I select three illustrations of poisons belonging to different classes.

(1) *Sulphuric Acid.*—If a person swallows *sulphuric acid*, the tissues with which the acid comes into contact are more or less charred:—

"more or less," that is, according to the strength of the acid and the time of contact.

Charred:—This implies a chemical act, dependent on the power of sulphuric acid to combine with water.

The portion of the body thus charred dies. We call this *molecular* death. (This does not imply that *the person* is dead. Health is disturbed. Health is derived from the old Saxon word "Wholth," signifying *entirety.* Health implies the perfect rhythmicity of the bodily functions. It is that condition expressed with charming simplicity by Suffolk folk, who describe being "quite well" by the phrase, "*they feel all over alike.*" The charring process (molecular death) has disturbed rhythmicity.) Before long all the members suffer with the charred stomach. The death, localised in the first instance, becomes general, the death of the entire body, i. e. of the person, eventually taking place. We call that *somatic* death. This is poisoning by sulphuric acid. But the primary act of disturbance—the first interference with the rhythmicity of health—resulted from the chemical power of sulphuric acid to combine with water. It will be evident that the chemical action of a poison depends on the chemical relationships of that poison.

(2) *Carbonic Oxide.*—Carbonic oxide is a true poison. It is a gas that may often be seen burning with a blue flame on the top of a bright fire in the open fire-stove.

Its importance amongst poisonous bodies depends on the circumstance that it is evolved in many manufacturing operations (e. g. lime and brick kilns, iron blast furnaces, copper-refining furnaces, &c.), and that it is always present in small quantity in coal gas, constituting its true toxic constituent.

What then is the mechanism whereby carbonic oxide destroys life?—The active agent of the blood is its red colouring matter (*Hæmoglobin*). To the chemist this substance abounds in wonder.

We have reason to believe that hæmoglobin is formed from the albumenoids, the synthesis of which albumenoids is limited to the vegetable. Essential as the albumenoids are to animal life, the animal is dependent for their formation on the synthetical processes taking place in the plant laboratory. The animal, however, can transmute one albumenoid into another (e. g. he can change albumen into a peptone), whilst he can also form from them bodies of less complicated constitution (e. g. fat):—in other words, he can lower them in the scale. But, save with one exception, he cannot use them to effect higher synthetical formations. This single exception is hæmoglobin.

It is no matter for surprise that a body like hæmoglobin—one of the chief actors, so to speak, in the curious drama of life and living—which comes on the scene through a stage opening, of which we neither know construction nor whereabouts—should possess unique chemical properties and relationships. I shall only trouble you this evening with one of these relationships.

As a general rule, a substance that combines with oxygen with difficulty, parts from it with ease, and *vice versâ*. It is difficult to make gold combine with oxygen, but it is easy to decompose oxide of gold. Potassium easily combines with oxygen, but it required the genius of a Davy, and the resources of the Royal Institution, to separate potassium and oxygen.

In hæmoglobin, however, we have a substance that combines with, and delivers up, its oxygen (i. e. is oxidized and reduced) with almost equal facility under similar conditions. Upon this and other chemical characteristics of hæmoglobin—as the oxygen-receiver, the oxygen-carrier, the oxygen-deliverer, the carbonic-acid receiver, carrier, and deliverer—the act of living depends. In other words, life depends on *the integrity* of the hæmoglobin—on the rhythmicity of those chemical processes, in effecting which hæmoglobin is the primary worker.

With these facts before us, let us turn to the toxic action of carbonic oxide.

The hæmoglobin at once seizes upon and combines with the carbonic oxide, carbonic-oxide-hæmoglobin being formed.

Two difficulties arise:

1. The hæmoglobin, saturated with carbonic oxide, cannot combine with oxygen. Regarding hæmoglobin as a *common carrier*, the carriage is full.

2. The hæmoglobin, being saturated with carbonic oxide, cannot get rid of the carbonic oxide under the ordinary conditions of respiration and circulation. Again, regarding the hæmoglobin as a common carrier, the vehicle, full up, cannot be unloaded.

To put all this in scientific phraseology, carbonic-oxide-hæmoglobin is a comparatively stable compound, being neither decomposed by the presence of an excess of oxygen (as in the lungs) nor by carbonic acid. What must happen? The man dies because the integrity of the hæmoglobin has been disturbed—because the normal sequence of its oxidation and reduction has been interrupted by the formation of carbonic oxide hæmoglobin.

We call the result of all these chemical actions and interferences, poisoning by carbonic oxide.

3. *Strychnine* (the poison derived from St. Ignatius' Bean).

How does strychnine act? We know sadly little about it—so little that we use the phrase "physiological action" to express our want of knowledge. But we know something.

A marked chemical characteristic of strychnine is its power to combine with oxygen when the oxygen is presented to it in a nascent form.

Note then the conditions. Strychnine is in the body. There is also present in the blood, hæmoglobin loosely combined with oxygen, which oxygen the hæmoglobin is always ready to give up on first demand. We are able to trace this action, and to see that the period of the primary strychnine fit coincides with the reduction of the hæmoglobin.

Why (you ask) does that kill? I cannot tell you. It is the highest form of knowledge to see the limits of positive knowledge, and to make that the starting-point for fresh inquiry.

From what I have said, it will be evident that my object has been to trace toxic energy to the chemical action of poisons on living tissues or fluids. The phrase "physiological action" must not be understood as implying any theory *re modus operandi.* There is a danger lest the phrase "physiological action" should be employed, or regarded, as explanatory. It no more explains (be it remembered) the action of certain drugs on the living body than the word catalysis explains fermentation.

There naturally follows on what I have said respecting this chemical action of poisons, the following important question:—

Given knowledge of certain properties of the elements, such as their atomic weights, their relative position according to the periodic law, their spectroscopic characters, &c.;—or given knowledge of the chemical composition, the molecular constitution, together with the general chemical and physical properties of compounds, in other words, given such knowledge of the element or compound as may be learnt in a laboratory—does such knowledge afford any clue whereby we may predicate the probable action of the element or of the compound respectively, on the living body?

1st. Let us limit our attention to the *elements.*

The starting-point of this inquiry was the toxic properties of the metals. The work of Blake (1841) in this direction was afterwards extended by Rabuteau (1867). Their observations led them to the general conclusion, that "the physiological activity of the metals increased with their atomic weight." This broad general statement was modified at a later period by noting that the reverse was the case with certain groups of metals. Thus potassium (39) is more poisonous than sodium (23), and barium (137) more poisonous than calcium (41). These facts led Rabuteau to the conclusion, that any comparisons of toxicity must be limited to the metals belonging to the same group. Husemann and Richet, however, pointed out that even this rule did not hold good, seeing that lithium having an atomic weight of 7, was far more poisonous than either sodium or potassium.

Experiments on the metals were further conducted by Richet with the metallic chlorides. Grain by grain, at intervals of forty-eight hours, he added the chlorides to water in which he kept fish of a given kind. He then recorded the maximum strength of the solution of the metallic chloride in which these said fish would live for forty-eight hours. The conclusion at which he arrived was that the limits of the toxicity of a metal bore no relationship either to its atomic weight, or to any other chemical or physical characteristic of the metal.

Bouchardat and Stewart Cooper, in a similar manner, experimented with the non-metals. Selecting the haloid group of elements, they

noted that their toxicity was inversely to their atomic weight, fluorine (19) being the most poisonous, and iodine (127) the least poisonous of the group, chlorine (35·5) and bromine (80) occupying their proper intermediate positions. But here again the group theory was inevitable. What was true of monad elements was not true of the elements of higher atomicity, the toxicity of selenium (79) being far greater than that of sulphur (32).

With these facts before us there arises this question, Was a relationship to be expected between physiological action and atomic weight? One poison acts on muscles—a second on nerves and nerve-centres—a third on the blood:—Is it likely, even supposing a relationship to exist between a certain group of elements and a given organ or a given structure, that the relationship would be the same in the case of all organs and all structures? These researches (the outline of which I have briefly indicated) suggest this much to future observers, viz.: First of all group your poisons according to their methods of operation, and then see how far the degree of toxicity of the terms of any one group show relationship to the atomic weights of such group.

But the difficulties of comparison thicken, when we consider the physiological action of certain allotropic modifications of the elements.

Thus compare *yellow* phosphorus, a body readily inflammable, soluble in bisulphide of carbon, firing by contact with iodine, with *red* phosphorus, a body at variance with the yellow variety in the three respects named. Nor is this all. For *yellow* phosphorus is an active poison, two grains being a certainly toxic dose, whilst *red* phosphorus is an absolutely inert body.

Take a second illustration. In its ordinary form oxygen plays the part of a life sustainer. But oxygen is only a life sustainer in its common form and at ordinary pressure.

On this latter point I have no time to dwell, save to mention that if an animal be exposed to oxygen at three pressures, the resulting symptoms are not unlike those induced by strychnine.

There is, however, an allotropic form of oxygen called *ozone*. The physiological action of ozone was the subject of a communication to the Royal Society of Edinburgh by Dewar and McKendrick (1873). Their experiments were made on both cold and warm-blooded animals, including amongst the latter themselves and their assistants.

The results are remarkable, more particularly when we remember that the air with which they operated, at most only contained 10 per cent. of ozone.

Placing a large frog in a jar of air, and then ozonizing the air, the frog in about half a minute closed its eyes, the respirations fell from 96 to 8 per minute, and the body temperature was lowered 4° or 5° C. The animal recovered in about 8 minutes, when pure air was admitted into the receiver. Death resulted if the animal was exposed for any lengthened period to the action of the ozonized air.

As regards the action of ozone on warm-blooded animals, Dewar

records certain personal experiences, chief amongst which were a tendency to breathe slowly, an enfeebled pulse, and fits of sneezing.

But now comes the curious part of the story, viz. that at the *post-mortem* on the animals that died under the influence of ozone, the blood was found to be *venous.* (The results were similar when pure ozonized oxygen was employed.) It is most remarkable that the *post-mortem* appearances of death from an intensified oxygen should resemble those of death from carbonic acid.

I give these illustrations to show why there should be reason to doubt whether the physical or chemical properties of an element can ever suggest either toxic activity or physiological action.

2nd. *Compounds.*

Is there any relationship between the chemical composition or constitution of a compound body and its physiological action?

The first series of researches on this question was directed to determining whether, in the case of a salt, the acid or the base was, physiologically, the most important ingredient (Blake 1841).

No doubt most often the active agent of a salt is the base, but this is by no means uniformly the case. Probably the solubility of a compound and the different proportions of acid to base in the salt (i. e. whether the compound in question be an acid or a basic salt) are agencies which also help to determine the toxicity of the body and its physiological action.

A second series of experiments was made by Blake for the purpose of showing that, given a series of isomórphic salts, the intensity of physiological action increased with the molecular weight. He further contended that salts crystallising in different forms had different physiological actions. On this basis he constructed a series of nine groups of salts, each group being characterised by special physiological actions, insisting with much reason that we possess in living matter a reagent (so to speak) capable of aiding us in our investigations into the molecular properties of chemical compounds. If from molecular constitution you can determine physiological action, probably from physiological action, conversely, you may determine molecular constitution.

Another series of experiments in a similar direction were made by Schoff on the Continent, and by Fraser and Crum Brown in this country.

Of these experiments the most important are those indicating how from bodies of vastly different physiological action you may obtain derivatives having similar properties.

For example, the physiological action of *strychnine* is primarily exerted on the spinal cord. As a result, convulsions occur as a prominent symptom. But if we introduce into the strychnine molecule a methyl group (forming methyl-strychnine), the action of the drug is altered—methyl-strychnine paralysing (strychnine stimulating) the motor nerves.

But here comes a curious fact. If we take morphine, or nicotine,

or atropine, or quinine, or veratrine (none of which bodies are comparable in their physiological action to strychnine), and convert them into their methyl derivatives, the methyl compounds formed (viz. methyl-morphine, methyl-nicotine, &c.) are comparable in their physiological action to methyl-strychnine.

We must admit these experiments to be striking. One treasures any experiment suggestive of the chemical constitution of a body indicating physiological action.

But again:—The true physiological action of a drug is not so much its general as its selective action, this selective action being largely dependent on the *dose* administered and the mode of administration.

For example: Inject into the circulation of a frog a *small* dose of *veratrine*, great muscular stiffness results, a *large* dose similarly administered not producing this effect. And now change the method of administration: Apply the *small* dose directly to the muscle, you get no symptom; but apply the *large* dose directly, and great muscular stiffness results. Here see the modifying influence of dose and of the mode of administration.

Again, the difficulty of "allotropism" in the case of elements, finds its counterpart in "isomerism" in the case of compounds. Thus cyanogen and paracyanogen are bodies of identical *percentage* composition, and yet cyanogen is one of the most poisonous of gases, whilst paracyanogen is one of the most inert of solids.

Or, again, take *piperin* and *morphine*. These bodies are of identical percentage and molecular composition. They agree (it is true) in being poisons. But how vastly different their physiological action! —the one an extreme irritant, the other a powerful narcotic.

I fear we must admit that, as no *a priori* reasoning could predict that by combining copper and sulphuric acid a blue salt would be formed, so no *a priori* reasoning, no knowledge of chemical constitution, can predicate what will be the special organ on which any given poison will act, nor, even supposing that the organ upon which the chemical activity of the drug will be exerted be known, what will be the nature of such chemical action. The science of drugs, like the science of chemistry, is, and must ever remain, an experimental science.

And, be it remembered, the poisons of the toxicologist are the medicines of the physician. Physiological action is a subject-matter for experiment. Let the guard be jealously set and as rigidly maintained to prevent cruelty to animals; but ask yourselves, whether to rob the higher creation of life and health rather than that one of the lower creation should suffer, be not a refinement of cruelty—the cruelty of cruelties? "Are ye not of much greater value than they?" speaks a still small voice amidst the noisy babble of well-intentioned enthusiasts.

Two general observations are suggested. And this first: The later age history of poisoning is the history of a profession. This

profession we find closely associated, not only with the profession of medicine (the art of healing), but with witchcraft, incantation, and charms. The threefold arts of poisoning, witchcraft, and medicine, moreover, became so closely allied to religion, as to claim, each and all, the shield of a sacred sanction and the protection of a Divine voice. Even that very word *φάρμακις*, the Greek for '*a dispenser of medicines*,' is the same word used to imply '*a witch*' and '*a poisoner*.' The modern scientist has once and for ever shattered the bond that united science with superstition. It was a special ministry of science to teach men that in the preparation of medicines the pharmacist required no stuffed crocodile to preside over the mysteries of his laboratory, nor incantation to give virtue to his drugs!

And this secondly: The villanies of the early poisoners can never again be practised in the light of the science of the nineteenth century. Science can and has done what legislation could never do. The Hebrew Scriptures speak of a time when "the sucking child shall play on the hole of the asp, and the weaned child shall put his hands on the basilisk's den" (Is. xi. 8, Rev. Ver.). Is not science working out some such consummation as this? I claim that a science which, like a blood-hound, can track with cunning scent the minutest atom of a poison in the body, *is* helping forward the day when poison shall cease to be the instrument of a secret treachery, because there are eyes it cannot hope to evade, and a science whose investigations it will not dare to defy.

[C. M. T.]

GENERAL MONTHLY MEETING,

Monday, March 5, 1888.

Edward Woods, Esq. M. Inst. C.E. Vice-President, in the Chair.

Frederick Beer, Esq.
Thomas Buckney, Esq. F.R.A.S.
The Hon. Justice Day,
J. E. Drower, Esq.
George Beloc Ellis, Esq.
David Charles Guthrie, Esq.
Robert George Hobbes, Esq.
Graham Hutchison, Esq. J.P.
Mrs. William Moir,
Percival Arthur L. Pryor, Esq.
Vincent Joseph Robinson, Esq.
Alfred Richard Sennett, Esq.
Louis Sterne, Esq.
George Philip Willoughby, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research:—

Professor Dewar, £100.

The following Arrangements for the Lectures after Easter were announced:—

Charles Waldstein, Esq. M.A. Ph.D.—Three Lectures on John Ruskin; on Tuesdays, April 10, 17, 24.

Walter Gardiner, Esq. M.A.—Three Lectures on The Plant in the War of Nature; on Tuesdays, May 1, 8, 15.

Sidney Colvin, Esq. M.A.—Three Lectures on Conventions and Conventionality in Art; on Tuesdays, May 22, 29, June 5.

Professor Dewar, M.A. F.R.S. *M.R.I.* Fullerian Professor of Chemistry, R.I. —Six Lectures on The Chemical Arts; on Thursdays, April 12, 19, 26, May 3, 10, 17.

Professor T. G. Bonney, D.Sc. LL.D. F.R.S.—Three Lectures on The Growth and Sculpture of the Alps (The Tyndall Lectures); on Thursdays, May 24, 31, June 7.

Carl Armbruster, Esq.—Seven Lectures on The Later Works of Richard Wagner (With Vocal and Instrumental Illustrations); on Saturdays, April 14, 21, 28, May 5, 12, 19, 26.

Professor C. E. Turner, of the University of St. Petersburg.—Three Lectures on Count Tolstoi as Novelist and Thinker; on Saturdays, June 2, 9, 16.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz :—

FROM

The Governor-General of India—Geological Survey of India: Palæntologia Indica, Ser. X. Vol. IV. Part 3. 4to. 1887.
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The French Government—Documents Inédits sur l'Histoire de France: Lettres de Catherine de Medicis. Par Cte. Hector de la Ferrière. Tome III. 1567-70. 4to. 1887.
Abel, Sir Frederick, C.B. D.C.L. F.R.S. M.R.I. (the Author)—Accidents in Mines. (Proc. Inst .Civil Eng. 1886-8.) 8vo. 1888.
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Canada Geological and Natural History Survey—Catalogue of Canadian Plants, Part III. Apetalæ. By J. Macoun. 8vo. 1886.
Chemical Society—Journal for February, 1888. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, 1887, Part 6a; 1888, Part 1. 8vo.
Editors—American Journal of Science for February, 1888. 4to.
Analyst for February, 1888. 8vo.
Athenæum for February, 1888. 4to.
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Chemist and Druggist for February, 1888. 8vo.
Engineer for February, 1888. fol.
Engineering for February, 1888. fol.
Horological Journal for February, 1888. 8vo.
Industries for February, 1888. fol.
Iron for February, 1888. 4to.
Murray's Magazine for February, 1888. 8vo.
Nature for February, 1888. 4to.
Revue Scientifique for February, 1888. 4to.
Scientific News for February, 1888. 4to.
Telegraphic Journal for February, 1888. 8vo.
Zoophilist for February, 1888. 4to.
Franklin Institute—Journal, No. 746. 8vo. 1888.
Geographical Society, Royal—Proceedings, New Series, Vol. X. No. 2. 8vo. 1888.
Geological Society—Quarterly Journal, No. 173. 8vo. 1888.
Harden, Edward B. Esq. (the Author)—The Construction of Maps in Relief. 8vo. 1887.
Linnean Society—Journal, Nos. 130, 137, 138. 8vo. 1887.
Manchester Geological Society—Transactions, Vol. XIX. Part 13. 8vo. 1887.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quinta, Vol. I. No. 12. 8vo. And Disegni. fol. 1887.
Montpellier Academie des Sciences et Lettres—Mémoires, Tome XI. Fasc. 1 (1885-6). 4to. 1887.

New York Academy of Sciences—Transactions, Vols. I. II. and V. 8vo. 1881-6.
Numismatic Society—Chronicle and Journal, 1887, Part 4. 8vo. 1887.
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Calendar for 1888. 8vo.
Photographic Society—Journal, Vol. XII. No. 4. 8vo. 1887.
Rio de Janeiro Observatory—Revista, No. 1. 8vo. 1888.
Annuario, 1885, 1886, 1887. 16to.
Royal Society of Edinburgh—Transactions, Vol. XXX. Part 4; Vol. XXXII. Parts 2–4; Vol. XXXIII. Part 1. 4to. 1883–7.
Proceedings, Nos. 121–123. 8vo. 1885–6.
Royal Society of London—Proceedings, Nos. 261, 262. 8vo. 1887–8.
Philosophical Transactions, Vol. CLXXVIII. 4to. 1888.
Science and Education Library, South Kensington Museum—Catalogue of Scientific Periodicals. 8vo. 1886.
St. Petersbourg, Academie Impériale des Sciences—Memoires, Tome XXXV. Nos. 8, 9. 4to. 1887.
Society of Arts—Journal, February, 1888. 8vo.
Statistical Society—Journal, Vol. L. Part 4. 8vo. 1887.
Telegraph Engineers, Society of—Journal, No. 69. 8vo. 1887.
United Service Institution, Royal—Journal, No. 142. 8vo. 1888.
United States Geological Survey—Mineral Resources of the United States for 1886. 8vo. 1887.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1888: Heft 1. 4to.
Wild, Dr. H. (the Director)—Annalen der Physikalischen Central-Observatoriums, 1886, Theil 2. 1887.
Wright and Co. Messrs. John (the Publishers)—The Medical Annual, 1888. 8vo.
Yorkshire Archæological and Topographical Association—Journal, Part 39. 8vo. 1888.

WEEKLY EVENING MEETING,

Friday, March 9, 1888.

Edward Woods, Esq. M. Inst. C.E. Vice-President, in the Chair.

Leslie Stephen, Esq. M.A.

*S. T. Coleridge.**

In the period which intervened between the Great War and the first Reform Bill, there were two centres of intellectual light in England. Jeremy Bentham, in his cheerful old age, reached his eightieth birthday in 1828, still, as he phrased it, codifying like any dragon, solving all problems by the application of his famous formula about the greatest happiness of the greatest number, and adding day by day to the vast piles of manuscript which were to embody the principles of all future legislation. To his hermitage in Westminster were admitted a little group of chosen disciples, the stern political economists, rigid utilitarians, and energetic reformers, some of whom were in the coming years to assume the title of philosophical radicals. Another band of enthusiasts sought a different shrine. They listened to an oracle which taught them that utilitarianism was "moral anarchy," political economy a "solemn humbug," radicalism the direct road to ruin, and true wisdom only to be found in regions of contemplation which Bentham could never enter—for a reason analogous to that which forbids pachydermatous quadrupeds to soar into the empyrean. We know pretty well what was the manner of man at whose feet these disciples sat. The keenest of contemporary observers has left a picture which must be laid under contribution for every description of Coleridge. Carlyle saw an old man—though in point of actual years he was Bentham's junior by nearly a quarter of a century—with the brow of a philosopher and the eye of a poet, but with the irresolute flabby mouth of a sensuous dreamer of dreams, consuming cups of tea, lukewarm but better than he deserved, or strolling, corkscrew fashion, along both sides of a garden path, unable to make up his mind to either. You put him a question; he replied by accumulating "formidable apparatus, logical swim-bladders,

* It seems desirable to say that some of the statements in the Lecture rest upon an examination of original documents, many of which have not hitherto been accessible to biographers. I owe my knowledge of them chiefly to Mr. Dykes Campbell, whose knowledge of the subject is most minute and exhaustive. A complete biography still remains to be written; it may be expected from Mr. Ernest Coleridge, who is in possession of his grandfather's MSS.—Leslie Stephen.

transcendental life-preservers, and other precautionary and vehiculatory gear for setting out;" but rambled into the universe at large, treated you "as a mere passive bucket, to be pumped into" (fancy a Carlyle for a passive bucket!), and finally left you "swimming and fluttering in the mistiest wide unintelligible deluge of things, for the most part in a rather profitless uncomfortable manner." Yet, at times, we are told, "balmy sunny islets, islets of the blest and intelligible," would rise out of the haze; and upon these islets the enthusiastic Sterling and others would try to cast anchor. Had they reached the solid foundation of creation, or had they, like Milton's pilot of the small night-foundered skiff, mistaken some metaphysical Kraken for the permanent framework of things?

That question may be answered dogmatically by any one who pleases. Immovable limits of time and capacity forbid me from attempting to answer it now. My excuse for venturing to say something of Coleridge—certainly one of the most fascinating and most perplexing figures in our literary history—is simply this: I have been forced to investigate with some care the details of his career; and I ought to be able not only to answer the question but to provide a little "vehiculatory gear" towards answering it. Coleridge's philosophy must of course be judged by considerations extraneous to his personal history. Yet I think, as a professional biographer is in duty bound to think, that philosophy is, more often than philosophers admit, the outcome of personal experience; and Coleridge's singular history may throw some light upon his teaching. Here we meet the hagiologist and the iconoclast, the twin plagues of the humble biographer. The hagiologist burns incense before his idol till it is difficult to distinguish any fixed outline through the clouds of gorgeously-tinted vapour. Coleridge thought himself to have certain failings. His relations fully agreed with him. His worshippers regard these meek confessions as mere illustrations of the good man's humility, and even manage to endow the poet and philosopher with all the homely virtues of the respectable and the solvent. To put forward such claims is to challenge the iconoclast. He, a person endowed by nature with a fine stock of virtuous indignation, has very little trouble in picturing the poet-philosopher as a shambling, unreliable, indolent voluptuary, to whom an action became impossible so soon as it presented itself as a duty, and who, even as a man of genius, must be condemned as unfaithful to his high calling. And so we raise the usual edifying discussion as to the privileges of genius. Do they include superiority to the Ten Commandments? Can you expect a poet to confine himself to one wife? May a man neglect his children because he has written the 'Ancient Mariner' and 'Christabel'?—points of casuistry, of which, with your leave, I will postpone the consideration to a future occasion.

For my purpose, it is enough to ascertain the facts. I have not to decide whether Coleridge should receive excommunication or canonisation; whether he deserved to go straight to heaven or to

pass a period—and, if so, how long a period—in purgatory. It is difficult to settle such questions satisfactorily. I desiderate an accurate diagnosis, not a judicial sentence. Coleridge sinned and repented. I take note of sin and of repentance as indications of character. I do not pretend to say whether in the eye of Heaven the repentance would be an adequate set-off for the sin. But I premise one apology for anything that may sound iconoclastic, and which I think is worth the consideration of the amiable persons who undertake to rehabilitate soiled reputations. A man's weakness can rarely be overlooked without underestimating his strength. If Coleridge's intellect were, as De Quincey said in his magniloquent way, "the greatest and most spacious, the subtlest and most comprehensive, that has yet existed among men" (what a philosopher one must be to pronounce such a judgment!), why were the results so small? Because the ethereal soul was chained to a fleshly carcase. To deny this is to force us to assume that what he did was all that he could do. You must either exaggerate his actual achievements beyond all possible limits, or save your belief in his potential achievements, by admitting that his intellect never had fair play.

Let us consider the antecedents of the prophet of Highgate Hill. Was there ever a young man fuller of intellectual promise or of personal charm than the youth of twenty-five, who, in 1797, rambled through the Quantocks discussing and composing poetry with Wordsworth? Circumstances apparently unfavourable had only served to stimulate his intellectual growth. Separated from his family in infancy, to become one of the victims of our public school system—ill-fed, ill-nursed, and ill-taught at Christ's Hospital; urged upon the treadmill of a sound classical education by a rigid schoolmaster, he had assimilated with singular aptitude whatever intellectual food had drifted within his reach. He had caught glimpses of high metaphysical secrets; he had peered into the mysteries of medical practice; he had bolted a miscellaneous library whole; he had been infected with poetical enthusiasm by the study of that minute day-star, W. L. Bowles; and he had completed his training by falling desperately in love with the inevitable sister of a schoolfellow. It is a comfort to reflect that the best regulated systems of education break down somewhere. Coleridge, it would have seemed, ran every risk of being driven sheep-like along the dull highroad of Latin grammar. Nature had prompted him to leap the fences, to expatiate in the wide fields of intellectual and imaginative pasture, and to derive a keener zest for his nourishment from the knowledge that the indulgence was illegitimate. Cambridge, the mother of poets, received him with the kindness she has so often shown to her children. We—I speak as a Cambridge man—we flogged (or nearly flogged) Milton into republicanism; we disgusted Dryden into an anomalous and monstrous preference for Oxford; we bored Gray till, half stifled with academic dulness, he sought more cheerful surroundings in a country churchyard; we left Byron to the congenial society of his

bear; we did nothing for Wordsworth, except, indeed, that we took him to Milton's rooms, and there for once (it must really have done him some good) induced him to take a glass too much; and we, as nearly as possible, converted Coleridge into a heavy dragoon. We ordered him to bow the knee to Euclid, and to Newton's Principia, the only idols whose merits were altogether beyond his powers of appreciation, and by such kindness in disguise induced him to plunge into a precocious breach with the proprieties. A fellowship might have converted him into a solid Church and State don, an oracle of the Combination Room, and a sound judge of port wine. We sternly withheld the temptation. A reformer has to start in life as a rebel. Coleridge sympathised with the rebellious William Frend, who was being banished from Cambridge for excessive liberalism. He offered his youthful incense to Priestley, the "patriot and saint and sage"—so the young enthusiast called him—who was soon to be expelled by the exuberant loyalty of Birmingham from an ungrateful country. Though never a Jacobin, he became what, in some form or other, a young man ought to become—an enthusiast for the newest lights, a partisan of the ideas struggling to remould the ancient order and raise the aspirations of mankind. The Master of the College shook his reverend head, kindly enough at times, at the lad's vagaries, and forgave him even for that preposterous attempt to become a trooper which never enabled him, with all his subtlety of distinction, to form any clear conception of the difference between a horse's head and its tail. But he could not run in the regular track. He was thrown into the chaotic world to sink or swim by his unassisted abilities. No man had, in some ways, a better floating apparatus. The poetic vein, soon to manifest itself in his best work, was indeed still turbid with the alloy of didactic twaddle. But already he had the versatility, the inherent vitality of intellect, the power of embodying philosophic thoughts in poetic imagery, which made him unrivalled in monologue. He talked better, I am apt to think, with his chum, Charles Lamb, at the "Cat and Salutation," than he ever talked to his worshippers at Highgate Hill. A man is at his best before he is recognised. Coleridge's early letters and essays show the fulness and intellectual vigour, without the too elaborate and slightly sanctimonious circumgyrations, of his later effusions. And his genius was such as implied a double portion of the power of making friends, which, with most of us, wanes so lamentably as the years go by. Lamb, his earliest and latest friend, was already devoted to this brilliant schoolfellow; and if Lamb was an easy conquest, men of less conspicuously tender nature were equally attracted. He had only to meet Southey at Oxford to swear at once an eternal friendship—a friendship to be cemented by a regeneration of the world.

Coleridge was to be the Plato of a new society to be founded in the wilds of America. There a short and healthy space of daily toil was to provide all that was necessary for a band of poets and philosophers, too benevolent to care for separate property, and worthy

founders of an Arcadia of perfect simplicity, refinement, and equality. As for the Eves of the Paradise, were there not three Miss Frickers? Coleridge repelled for a time the too obvious foreboding that Pantisocracy was but a province of dreamland. Dreamland was his reality. For the demands of butchers and bakers he had still a lordly indifference. He had the voice which could charm even a publisher. The prim and priggish Cottle was at once annexed by Coleridge, and all the natural caution of a tradesman did not withhold him from promising a guinea for every hundred lines to be produced by a still untried new poet. What were one hundred lines to the genius which could turn off an act of a tragedy in a morning, and which soon afterwards could build the shady palace of Kubla Khan in a dream? Coleridge was justified, in point of bare prudence, in marrying at once on the prospect. Somehow the poetry did not come so fast as the bills. But Coleridge had other strings to his bow. He set up as a lecturer and journalist. His marvellous eloquence condescended for the nonce to wile promises of subscription even from dealers in tallow; and the philosopher—not without a humorous sense of his own absurdity—became a successful commercial traveller. The newspaper of course collapsed almost on the spot. All the arrangements were absurd, and Coleridge's eloquence proved to be somehow uncongenial to the tallow-dealing interest. But meanwhile, in the course of his journey, Coleridge had incidentally and, as it were, by the mere side glance of his eye, swept up Charles Lloyd, son of a rich banker, who, fascinated and enthralled, left the bank to become an inmate of his teacher's house, and, no doubt, a contributor to its expenses. Poole, a most public-spirited and intelligent man, offered him an asylum at Nether Stowey. The Unitarians, to whom he more or less belonged, were ready to open their pulpit to a preacher whose eloquence promised to rival even the most splendid traditions of the refined age of Leighton and Jeremy Taylor.

Hazlitt, not yet soured and savage, heard Coleridge preach in 1798; and tells us in true Hazlittian style how his voice rose like a storm of rich distilled perfumes; how he launched into his subject like an eagle dallying with the wind; how, in brief, poetry and philosophy had met together, truth and genius had embraced under the eye and with the sanction of reason. The Unitarian firmament was too cramped for this brilliant meteor; the philosophy expounded from the pulpits seemed to him meagre and rigid; and, while hesitating, he received an offer from the generous Wedgwoods, anxious to spend some part of their wealth in the patronage of genius.

Rumours had reached England by this time that a great intellectual light had arisen in Germany. The Wedgwoods gave Coleridge a modest annuity, unfettered (as I can now say) by any condition whatever, a fact which makes the subsequent withdrawal a harsher measure than has been supposed. Coleridge resolved to go to Germany, catch the sacred fire of the Kantian philosophy, and return to England to regenerate the mind of his countrymen. He started

in September, 1798, when he was just twenty-six, in company with the friend who alone could be compared to him in intellectual power. Wordsworth had been attracted, as Lamb and Southey had been attracted before him. Coleridge and Wordsworth had discussed the principles of their common art; and Coleridge had applied them in those wonderful poems, the 'Ancient Mariner' and 'Christabel' (the first part), which were to be but the prologue to a fuller utterance; a wonderful prologue, for, though followed by nothing, it remained unique and inimitable. Coleridge was not yet *déterré*, as Pope said of Johnson; the ordinary critics had only a passing smile or sneer for the little clique which published its obscure utterances in a provincial town. Monthly and critical reviewers—the arbiters of taste—would have been astonished to hear that Coleridge and Wordsworth and Lamb and Southey would soon stand in the very front ranks of English literature; and he must have a clearer conscience than I who would cast a stone at critics for not at once detecting the first germs of rising genius. But, as *ex post facto* prophets, we are able to see that Coleridge already had not only given proofs of astonishing power, but had won what was even more valuable, the true sympathy and cordial affection of young men who were the distinct leaders of the next generation. Even material support was not wanting from such men as Poole and Wedgwood sufficient to ensure a fair start for the little band of prophets. We should have been justified in foretelling, with unusual confidence, a career of surpassing brilliancy for the youth, of whom it seemed only questionable whether he would choose to be a second Bacon or a second Milton.

And if, at that time, any one could have shown us the same Coleridge at a distance of eighteen years, the worn, depressed, prematurely aged man who took up his abode with Gillman in 1816, we should have been shocked, and yet, perhaps, have been able to utter our complacent "I told you so." What so far had been the achievements of the most brilliant genius of the generation: a man not only of surpassing ability, but of surpassing facility of utterance; a man whom to set going at any moment was to unlock a perpetually flowing fountain of abounding eloquence? A few newspaper articles and some courses of lectures, he said in 1817, constituted his whole publicity. It may be added that he had jotted down on the margins of books enough detached thoughts to have made some volumes of admirable reflections. But he had achieved nothing to suggest concentrated thought or sustained labour. In a shorter period Scott poured out the whole of the Waverley novels, besides discharging official duties, and writing a number of reviews and miscellaneous works. I say nothing as to the quality. I am simply thinking of the amount of work; and Coleridge's work cost little labour, for his power of improvisation was among his most marvellous faculties. Why, then, was the work so limited in quantity? The internal facts are sufficiently significant. After his return from Germany in the autumn of 1799, he wrote some articles which

certainly proved that his intellect was in full vigour, translated 'Wallenstein,' and then, in 1800, retired with his family to Keswick. Here at once ominous symptoms begin to show themselves. A strange disquiet is betrayed in his letters; there are painful complaints of ill-health; his poetic inspiration breathes its last in the 'Ode to Dejection.' He sought in vain to distract painful thought by metaphysical abstractions; he rambled off in 1804 to spend two years and a half in Malta and Italy. Returning to England, he tried lecturing at the Royal Institution, and then settled at Grasmere—fifteen miles of mountain roads from his wife—and repeated his 'Watchman' experiment by writing the 'Friend.' The youthful buoyancy, even flippancy, has departed, though it shows far riper thought and richer intellectual stores. But weariness of spirit marks every page; the long sentences somehow suggest a succession of stifled groans; as the enterprise proceeds, it can only be kept up by introducing any irrelevant matter that may be on hand—such as old letters from Germany which happened to be in his portfolio, and an extravagant panegyric upon his patron at Malta, Sir Alexander Ball.

The 'Friend' soon falls dead, and Coleridge drifts back to London. There he makes efforts, pathetic in their impotence, to keep his head above water. He tries journalism again, but without the occasional triumphs which had formerly atoned for his irregularity. He lectures, and is heard with an interest which shows that, in spite of all impediments, his marvellous powers have at least roused the curiosity of all who claim to have an intellectual taste. He has a gleam of success, too, from the production of his old tragedy, 'Remorse,' written in the days of early vigour. But some undertow seems to be sucking him back, so that he can never get his feet planted on dry land. He retires to Bristol, and thence to Calne, where he seems to be sinking into utter obscurity. He has almost passed out of the knowledge of his friends, when a last despairing effort lands him at Highgate, and there a rather singular transformation, it may seem at first sight, enables him to become the oracle of youthful aspiration, wisdom, and virtue. Painfully, and imperfectly with their aid, he gathers together some fragments of actual achievement—enough to justify a great, but a most tantalising reputation.

What was the secret of this painful history? Briefly, it was opium. Coleridge said so himself, and all his biographers have stated the facts. Without this statement the whole story would be unintelligible, and we could have done justice neither to Coleridge's intellectual powers nor even to some of his virtues. To tell the story of Coleridge without the opium is to tell the story of Hamlet without mentioning the ghost. The tragedy of a life would become a mere string of incoherent accidents. Nor are the facts doubtful. Coleridge, I fear, composed, or invented, for the benefit of Gillman, a certain picturesque "Kendal black drop"—a treacherous nostrum,

it is suggested, which gave him relief in his sufferings at Keswick, and overpowered his will before he had recognised its nature. The truth is, as can be abundantly proved by his letters at the time, that he was taking laudanum in large quantities in 1796, that is when he was just twenty-four, under the pressure of illness, but certainly well knowing what he was taking. It was at Keswick, not that he first indulged, but that he first became aware of his almost hopeless enslavement.

After reading many painfully conclusive proofs of this passion, I confess that I think it less remarkable that his demoralisation in this respect seemed to be complete about 1814, than that he succeeded, under Gillman's care, in so far breaking off the habit as to make a certain salvage from the wreck. I simply take note of these facts, and leave anybody who pleases to do the moralising; but I am forced to add a few words upon another topic, to which his apologists have resorted in order to extenuate the opium-eating. Briefly, it has been attempted to save his character by abusing his wife. Undoubtedly, as the recently published Coleorton papers prove, there was a complete want of sympathy. The same documents show that it was not, as had been generally supposed, a case of gradual drifting apart. Proposals for a regular separation had been made by the time of Coleridge's return from Malta. Coleridge's apologists have said that Mrs. Coleridge was one of Iago's women, born "to suckle fools and chronicle small beer," and quite unable to appreciate Kantian metaphysics, or even 'Christabel.' A very doubtful legend has been put about, that she once said, "Get oop, Coleridge" (a remark for which one can conceive a sufficient justification), and no man can be expected to care for a woman who says "Get oop," or for her children. From letters of hers which I have seen, I am inclined to think that Mrs. Coleridge must really have been a very sensible woman, who worked hard to educate her own children, and the children of her sister, Mrs. Southey, in French and Italian, and who could express herself in remarkably good English. She was no doubt inappreciative of a genius which could not be set to bread-winning. And moreover, when a man has an ecstatic admiration for another woman, it is not likely to make his relations to his wife more pleasant. To speak of all this as a moral excuse for Coleridge is to my mind unmanly. If a man of genius condescends to marry a woman, and be the father of her children, he must incur responsibilities. The fact that he leaves her, as Coleridge did, his small fixed income, the balance of her expenses to be made up by his brother-in-law and other connections, is so far to his credit, but does not excuse him for a neglect of those duties, not to be measured in pounds, shillings, and pence, which a husband and father owes to an innocent woman and three small children. Coleridge's position was no doubt difficult, but the mode in which he solved the difficulty is a proof that opium-eating is inconsistent with certain homely duties.

An experienced person has said, "Do not marry a man of genius." I have no personal interest in that question, nor will I express any opinion upon it, but one is inclined to say, Don't be his brother-in-law, or his publisher, or his editor, or anything that is his if you care twopence—it is probably an excessive valuation—for the opinion of posthumous critics.

But, again, I would avoid moralising. I only ask what is the true inference as to Coleridge's character. And that consideration may bring us back to less painful reflections. It is preposterous to maintain the thesis that Coleridge was the kind of person to be held up as a pattern to young men about to marry. Opium had ruined the power of will, never very strong, and any capacity he may have had—and his versatility was perhaps incompatible with any great capacity—for concentration on a great task. The consequences of such indulgence had ruined his home life, and all but ruined his intellectual career. But there is also this to be said, that at his worst Coleridge was both loved and eminently lovable. His failings excited far more compassion than indignation. The "pity of it" expresses the sentiment of all eye-witnesses. He was always full of kindly feelings, never soured into cynicism. The strange power of fascination which he had shown in his poetic youth never deserted him. As De Quincey has said: "Beyond all men who ever perhaps have lived, he found means to engage a constant succession of most faithful friends. He received the services of sisters, brothers, daughters, sons, from the hands of strangers, attracted to him by no possible impulses but those of reverence for his intellect and love for his gracious nature. Perpetual relays were laid along his path in life of zealous and judicious supporters." Whenever Coleridge was at his lowest, some one was ready to help him. Poole, and Lloyd, and Wedgwood, and De Quincey, had come forward in their turn. Through the dismal years of degradation which preceded his final refuge at Gillman's, the faithful Morgans had made him a home; tried to break off his bad habits, and enabled him to carry on the almost hopeless struggle. When Morgan himself became bankrupt, it is pleasant to know that Coleridge, among whose faults pecuniary meanness had no place, gave what he could—and far more than he could really spare—to help his old friend. When he delivered his lectures or poured out an amazing monologue at Lamb's suppers, or in Godwin's shop, young men, at the age of hero-worship, were already prepared not only to wonder at the intellectual display, but to feel their hearts warmed by the real goodness shining through the shattered and imperfectly transparent vessel. Coleridge's letters may reveal some part of this charm, though some part, too, of the drawback. His long involved sentences, compared by himself to a Surinam toad with a brood of little toads escaping from his back, wind about in something between a spoken reverie and a sympathetic effusion of confidential confessions. When they touch the practical, e. g. publishers' accounts, they are apt to become hopelessly unintelligible.

When they expound a vast scheme for a *magnum opus,* or one of the various *magna opera* which at any time for thirty years were just ready to issue from the press, as soon as a few pages were transcribed, we perceive, after a moment, that they are not the fictions of the begging-letter writer, but a kind of secretion, spontaneously and unconsciously evolved to pacify the stings of remorse. There are moments when he is querulous, but we must forgive them to the man who had been hopelessly distanced in popular fame by his inferiors; whose attempts at public utterance had utterly collapsed; whose 'Wallenstein' still encumbered his publisher's shelves; whose poetical copyrights had been deliberately valued at *nil;* and whose name was only mentioned in the chief reviews as a superlative for wilful eccentricity and absurdity. And then, at every turn, we come upon frequent gleams, not only of subtle thought and imaginative expression, but of shrewd common sense, and even at times of a genuine humour, which seems to imply that Lamb was partly serious when he said that Coleridge had so much 'f-f-fun' in him. After reading many of the letters, which still remain unpublished, I may say that it is my own conviction that a life of Coleridge may still be put together by some judicious writer, who should take Boswell rather than the 'Acta Sanctorum' for his model, which would be as interesting as the great 'Confessions'; which should by turns remind us of Augustine, of Montaigne, and of Rousseau, and sometimes, too, of the inimitable Pepys or Boswell himself; which should show the blending of the many elements of a most complex character and a most versatile and opulent intellect; which should often call forth wonder, and smiles, and sighs, and indignation smothered by pity, in one of those unique combinations which it would take a Shakespeare to portray and act, and defy the skill of a psychologist to define.

Only a faint indication of this is to be found in Coleridge's 'Apologia,' or, as he called it, his 'Biographia Literaria,' of which I must now say a word. It was written at his very nadir, and published just after he had reached his asylum at Highgate. In this sense it has a special biographical value, though its statements, coloured by the illusions to which he was then specially subject, have passed muster too easily with his biographers. Its aim is chiefly to protest against the neglect of the public and the dispensers of patronage. Such complaints generally remind me of a rifleman complaining that the target persists in keeping out of the line of fire. But if we must pardon something to a man so grievously tried for endeavouring to shift a part of the responsibility upon other shoulders than his own, we must be upon our guard against accepting censures which involve injustice to others. Nothing but Coleridge's strange illusions could be an apology, for example, for his complaints that the Ministry had not rewarded a writer whose greatest successes had been scornful denunciations of their great leader, Pitt. The book, of course, is put together with a pitchfork. It is without form or proportion, and is finally eked out with a batch of the old letters from

Germany which he had already used in the 'Friend,' and apparently kept as a last resource to stop the mouths of printers.

Now it is remarkable that even at this time, when his demoralisation had gone furthest, he could still pour out many pages of criticism, quite irrelevant to the professed purpose of the book, and yet such as was beyond and above the range of any living contemporary. Coleridge at his worst lost the power of finishing and concentrating—of which he had never had very much—but not the power of discursive reflection. He must be compared not to a tree which has lost its vital fibre, but to a vine deprived of its props, which, though most of its fruit is crushed and wasted, can yet produce grapes with the full bloom of what might have been a superlative vintage. But there is one fact of the 'Biographia' for which the apology of illusion is more requisite even than for his misstatements of fact. Coleridge has often been accused of plagiarism. I do not believe that he stole his Shakespeare criticism from Schlegel, and, partly at least, for the reason which would induce me to acquit a supposed thief of having stolen a pair of breeches from a wild Highlandman. But it is undeniable that Coleridge was guilty of a serious theft of metaphysical wares. The only excuse suggested is that the theft was too certain of exposure to be perpetrated. But, as it certainly was perpetrated, this can only be an apology for the motive. The simple fact is that part of his scheme was to establish his claims to be a great metaphysician. But it takes much trouble and some thought to put together what looks like a chain of *a priori* demonstration of abstract principles. Coleridge, therefore, persuaded himself that he had really anticipated Schelling's thoughts and might justifiably appropriate Schelling's words. He threw out a few phrases about "genial coincidence"—perhaps the happiest circumlocution ever devised for what Pistol called "conveying"—and adopted Schelling in the lump. When he had come to an end of Schelling's guidance, he proceeded —with an infantile simplicity which disarms indignation—to write a solemn complimentary letter from himself to himself, pointing out that the public would have had enough of the discussion, and "Dear C." politely agreed to drop the subject, with proper compliments to his "affectionate, &c."

And now I come to the very difficult task of indicating, as briefly as I can, the bearing of these remarks upon Coleridge's multifarious activity. It is not possible to sum up in a few phrases the characteristics of a man who wrote upon metaphysics, theology, morals, politics, and literary criticism; who made a deep impression in all the departments of thought; whose utterances are scattered up and down in fragmentary treatises, in complex arguments which generally break off in the middle, and in miscellaneous jottings upon the margins of books; whose opinions have been differently interpreted by different disciples, and have in great part to be inferred from his comments upon other writers, and can only be intelligible when we have settled what those writers meant, and what he took them to

mean; who frequently changed his mind, and who certainly appears, to thinkers of a different order, to add obscurity even to subjects which are necessarily obscure. Nor is the difficulty diminished when, as in my case, the commentator belongs to what must be called the antagonistic school, and is even most properly to be described as a thorough Philistine who is dull enough to glory in his Philistinism. All that I shall attempt is to select a certain aspect of the Coleridgian impulse, and to say what impression it makes upon a radically prosaic mind.

The brilliant Coleridge of Nether Stowey, the buoyant young poet-philosopher who had not yet been to Germany, was still a curious compound of imperfectly fused elements. His Liberalism had led him to the Unitarianism of Priestley and the associative philosophy of Hartley. But he had also dipped into Plotinus and into some of the mystical writers who represent the very opposite pole of speculation. The first doctrine was imposed upon him from without, the other was that which was really congenial to his temperament. For Coleridge was, above all, essentially and intrinsically a poet. The first genuine manifestations of his genius are the poems which he wrote before he was twenty-six. The germ of all Coleridge's utterances may be found—by a little ingenuity—in the 'Ancient Mariner.' For what is the secret of the strange charm of that unique achievement? I do not speak of what may be called its purely literary merits—the melody of versification, the command of language, the vividness of the descriptive passages, and so forth—I leave such points to critics of finer perception and a greater command of superlatives. But part, at least, of the secret is the ease with which Coleridge moves in a world of which the machinery (as the old critics called it) is supplied by the mystic philosopher. Milton, as Penseroso, implores

> The spirit of Plato to unfold,
> What worlds or what vast systems hold
> The spirit of man that hath forsook
> Her mansion in this fleshy nook,
> And of those demons that are found
> In fire, air, flood, and underground,
> Whose powers have a true consent
> With planet and with element.

If such a man fell asleep in his "high lonely tower," his dreams would present to him in sensuous imagery the very world in which the strange history of the 'Ancient Mariner' was transacted. It is a world in which both animated things, and stones, and brooks, and clouds, and plants are moved by spiritual agency; in which, as he would put it, the veil of the senses is nothing but a symbolism everywhere telling of unseen and supernatural forces. What we call the solid and the substantial becomes a dream; and the dream is the true underlying reality. The difference between such poetry, and the

poetry of Pope, or even of Gray, or Goldsmith, or Cowper—poetry which is the direct utterance of a string of moral, political, or religious reflections—implies a literary revolution. Coleridge, even more distinctly than Wordsworth, represented a deliberate rejection of the canons of the preceding school; for, if Wordsworth's philosophy differed from that of Pope, he still taught by direct exposition instead of the presentation of sensuous symbolism. The distinction might be illustrated by the ingenious criticism of Mrs. Barbauld, who told Coleridge that the 'Ancient Mariner' had two faults—it was improbable, and had no moral. Coleridge owned the improbability, but replied to the other stricture that it had too much moral, that it ought to have had no more than a story in the 'Arabian Nights.' Indeed, the moral, which would apparently be that people who sympathise with a man who shoots an albatross will die in prolonged torture of thirst, is open to obvious objections.

Coleridge's poetical impulse died early; perhaps, as De Quincey said, it was killed by the opium; or as Coleridge said himself, that his afflictions had suspended what nature gave him at his birth,

> His shaping spirit of imagination.

So that his only plan was

> From his own nature all the natural man,
> By abstruse research to steal,

and partly, too, I should guess, for the reason that this strange mystic world in which he was at home, was so remote from all ordinary experience that it failed even to provide an efficient symbolism for his deepest thoughts, and could only be accessible in the singular glow and fervour of youthful inspiration. The domestic anxieties, the pains of ill-health, the depression produced by opium, were a heavy clog upon an imagination which should try to soar into vast aërial regions. But it may be doubtful whether this peculiar vein of imagination, emptied in the 'Ancient Mariner' and 'Christabel,' could in any case have been worked much further.

At any rate, Coleridge, as his imaginative impulse flagged, passed into the reflective stage; and, as was natural, his mind dwelt much upon those principles of art which he had already discussed with Wordsworth in his creative period. In saying that Coleridge was primarily a poet I did not mean to intimate that he was not also a subtle dialectician. There is no real incompatibility between the two faculties. A poetic literature which includes Shakespeare in the past and Mr. Browning in the present is of itself a sufficient proof that the keenest and most active logical faculty may be combined with the truest poetical imagination. Coleridge's peculiar service to English criticism consisted, indeed, in great measure, in a clear appreciation of the true relation between the faculties, a relation, I think, which he never quite managed to express clearly. Poetry, as he says,

is properly opposed not to prose but to science. Its aim, he infers, is not to establish truth but to communicate pleasure. The poet presents us with the concrete symbol; the man of science endeavours to analyse and abstract the laws embodied. Shakespeare was certainly not a psychologist in the sense in which Professor Bain is a psychologist. He does not state what are our ultimate faculties, or how they act and react, and determine our conduct; but, so far as he creates typical characters, he gives concrete psychology, or presents the problems upon which psychology has to operate. Therefore, if poetry, as Coleridge says after Milton, should be simple, sensuous, passionate, instead of systematic, abstract, and emotionless, like speculative reasoning, it is not to be inferred that the poet should be positively unphilosophical, nor is he the better, as some recent critics appear to have discovered, for merely appealing to the senses as being without thoughts, or, in simpler words, a mere animal. The loftiest poet and the loftiest philosopher deal with the same subject-matter, the great problems of the world and of human life, though one presents the symbolism and the other unravels the logical connection of the abstract conceptions.

Coleridge, having practised, proceeded to preach. That a poet should also be a good critic is no more surprising than that any man should speak well on the art of which he is master. Our best critics of poetry, at least, from Dryden to Mr. Matthew Arnold, have been (to invert a famous maxim) poets who have succeeded. Coleridge's specific merit was not, as I think, that he laid down any scientific theory. I don't believe that any such theory has as yet any existence except in embryo. He was something almost unique in this as in his poetry, first because his criticism (so far as it was really excellent) was the criticism of love, the criticism of a man who combined the first simple impulse of admiration with the power of explaining why he admired; and secondly, and as a result, because he placed himself at the right point of view; because, to put it briefly, he was the first great writer who criticised poetry as poetry, and not as science. The preceding generation had asked, as Mrs. Barbauld asked: What is the moral? Has *Othello* a moral catastrophe? What does 'Paradise Lost' prove? Are the principles of Pope's 'Essay on Man' philosophical, or is Goldsmith's 'Deserted Village' a sound piece of political economy? The reply embodied in Coleridge's admirable criticisms, especially of Shakespeare, was that this implied a total misconception of the relations of poetry to philosophy. The "moral" of a poem is not this or that proposition tagged to it or deducible from it, moral or otherwise; but the total effect of the stimulus to the imagination and affections, or what Coleridge would call its dynamic effect. That will, no doubt, depend partly upon the philosophy assumed in it; but has no common ground with the merits of a demonstration in Euclid or Spinoza. It is this adoption of a really new method, which makes us feel when we compare Coleridge, not only with the critics of a past generation, but even with very able and

acute writers such as Jeffrey or Hazlitt, who were his contemporaries, that we are in a freer and larger atmosphere, and are in contact with deeper principles. It raises another question, for it leads to Coleridge's most conscious aim. Nothing is easier than to put the proper label on a poet—to call him "romantic," or "classical," and so forth; and then, if he has a predecessor of like principles, to explain him by the likeness, and if he represents a change of principles, to make the change explain itself by calling it a reaction. The method is delightfully simple, and I can use the words as easily as my neighbours. The only thing I find difficult is to look wise when I use them, or to fancy that I give an explanation because I have adopted a classification. Coleridge, both in poetry and philosophy, conceived himself to be one of the leaders of such a reaction. He proposed to abolish the wicked, mechanical, infidel, prosaic eighteenth century and go back to the seventeenth. I do not believe in the possibility or the desirability of any such reaction. I prefer my own grandfathers to their grandfathers, and myself—including you and me—to my grandfathers. I am quite sure that, if I did not, I could not make time run backwards. We are far enough off to be just to the maligned eighteenth century, and to keep all our uncharitableness for our contemporaries—it may do them some good. I would never abuse the century which loved common sense and freedom of speech, and hated humbug and mystery; the century in which first sprang to life most of the social and intellectual movements which are still the best hope of our own; in which science and history and invention first took their modern shape; the century of David Hume, and Adam Smith, and Gibbon, and Burke, and Johnson, and Fielding, and many old friends to whom I aver incalculable gratitude; but I admit that, like other centuries, it had its faults. It was, no doubt, unpoetical at its close—almost as unpoetical as the latter half of the nineteenth; and somehow it had fallen into that queer blunder of judging poetry by the canons of science. The old symbolism of an earlier generation had faded, and for Pagan or Christian imagery we had frigid personifications, such even as Coleridge quotes from some prize poem: "Inoculation, heavenly maid!" a deity who could be only adored in a rhymed medical treatise. And Coleridge's charge against the philosophy of the time was really identical with his charge against the poetry.

Poetry, without the mystic or spiritual element, meant Darwin's "Botanic Garden"—an ice-palace, as he called it, a heap of fine phrases and sham personifications. Take the same element from theology, and you have Paley's 'Evidences'; from morals, and the residuum is Bentham's utilitarianism. Coleridge's nomenclature expressed this in a fashion. He was fond of saying that all men were born Aristotelians or Platonists: Platonists, if, in his favourite distinction, the reason and the imagination dominated in them, and Aristotelians if they had only the understanding, the almost vulpine cunning, which was shared even by the lower animals, which meant prudence in morality, reliance upon mere external evidence in

theology, and pure expediency in politics. How the Aristotelians had come to rule the world ever since the opening of the eighteenth century is a question which, so far as I know, he never answered. But the effect of their dominion was equally to dethrone reason as to asphyxiate imagination. The two were allies, if not an incarnation of the same faculty. Inversely the Benthamites, till Mill was converted by Wordsworth, regarded poetry as equivalent to mere tintinnabulation and lying, or, as Carlyle's friend put it, the "prodooction of a rude age." It was as much in his character of poet as of philosopher, that Coleridge hated Political Economy, the favourite science of the Benthamites; for, according to him, it was an illustration of their destructive method. The economist deals with mere barren abstractions, and then misapplies them to the concrete organism, the life of which, according to the common metaphor, has been destroyed by his dissecting knife. Coleridge goes too far in speaking as if analysis were in itself a mischievous instead of an important process, much as Wordsworth thought that every man of science was ready to botanise on his mother's grave. But, on the other hand, the clear conviction that a society could only be explained as an organic and continuous whole enables him to point out very distinctly the limits of the opposite school. One indication of this contrast may be found in Coleridge's theory of Church and State. It is curious that Mill, in his essay upon Coleridge, especially admires him for taking into account the historical element in which Bentham was deficient. It is curious because it is remarkable that the leader of a school which boasted specially of resting upon experience, should admit that it was weak precisely in not appreciating the historical method on which surely experience should be founded. It seems almost as if the antagonists had changed weapons, like the duellists in *Hamlet*. The *a priori* thinker rests upon experience, and the empiricist upon a really *a priori* method.

The ambiguity indicates Coleridge's peculiar position towards the opposite school. He regards society as an organism, a something which has grown through long centuries, and therefore to be studied in its vital principle, not to be analysed into a mere mechanism for distributing certain lumps of happiness. In doing so he was saying what had been said by Burke, whose wisdom he fully appreciated and whose real consistency he recognised. To my mind, indeed, Burke as a political philosopher was far greater than Coleridge. But Burke hated the metaphysics in which Coleridge delighted, and therefore with him we seem at best to come upon blank prejudice, or prescription, as the ultimate ground of political science. Coleridge feels the necessity of connecting his organic principles with some genuine philosophical principle, and Mill admits that Conservatism in his treatment was something very superior to the mere brute prejudice to which Eldon and Castlereagh appealed, and which was used as a bludgeon by *The Quarterly Review*. Unluckily it is here, too, that we find the weakness of Coleridge's character. He tried

to put together his views at a time when his mind had been hopelessly enervated; when he could guess and beat about a principle, but could never get it fairly stated or see its full bearings. He is struggling for utterance, still clinging to the belief that he can elaborate a system, but never getting beyond prolegomena and fruitful hints. He says that to study politics with benefit we must try to elaborate the "idea" of Church and State, and the "idea," as he explains, is identical with what scientific people call a law. But how the law or laws of an organism are to be determined by some transcendental principle overruling and independent of experiences, is just the point which remains inexplicable. He seems to appreciate what we now call the historic method. He uses the sacred phrase 'evolution,' which is simply the general formula of which the historic method is a special application. But we find that by evolution he means some strange process suggestive of his old mystical employment, and even at times talks of heptads and pentads and the "adorable tetractys," which is the same with the Trinity; and connects chemical laws of oxygen and hydrogen gas with the logical formulæ about prothesis, and antithesis, and mesothesis. To state the theory of evolution in verifiable and scientific terms was reserved for Darwin; when we meet it in Coleridge we seem to be going back to Pythagoras; and yet it is the same thought which is struggling for an utterance in singular and bewildering terms, and moreover it was just the theory which Mill required.

But, to come to a conclusion, though I cannot think that Coleridge ever worked with his mind clear, or was, indeed, capable of the necessary concentration and steadiness of thought by which alone philosophical achievements are possible; though I hold, again, that if he had succeeded he would have found that he was not so much refuting his opponents as supplying a necessary complement to their teaching, I can still believe that he saw more clearly than any of his contemporaries what were the vital issues; that in his detached, and desultory, and inconsistent fashion he was stirring the thoughts which were to occupy his successors; and that a detailed examination would show in how many directions a certain Coleridgian leaven is working in later fermentations.

Besides the able and zealous disciples who acknowledged his leadership, we might find many affinities in Carlyle's masculine if narrow teaching; or again, in a school which diverged in a very opposite direction, for the theory of Church authority sanctioned by the Oxford disciples of Cardinal Newman is, in spite of its different result, closely allied to Coleridge's; while the modern Hegelians—though they regard him as a superficial dabbler—must admit that he rendered the service (of doubtful value, perhaps) of infecting English thought with the virus of German metaphysics, and will perhaps admit that, in principle, he anticipated some of their most cogent criticisms of the common enemy. Coleridge never constructed a system. If a philosophy, or its creator, is to be judged by the

systematic characters, Coleridge must take a very low place. But when we think what philosophical systems have so far been; what flimsy and air-built bubbles in the eyes of the next generation; how often we desire, even in the case of the greatest men, that the one vital idea (there is seldom so much as one!) could be preserved, and the pretentious structure in which it is involved permitted once for all to burst; we may think that another criterion is admissible; that a man's work may be judged by the stimulus given to reflection, even if given in so intricate a muddle and such fragmentary utterances that its disciples themselves are hopelessly unable to present it in an orderly form. Upon that ground, Coleridge's rank will be a very high one, although, when all is said, the history, both of the man and the thinker, will always be a sad one—the saddest in some sense that we can read, for it is the history of early promise blighted and vast powers all but running hopelessly to waste.

[L. S.]

WEEKLY EVENING MEETING,

Friday, March 16, 1888.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

John Murray, Esq.

Structure, Origin, and Distribution of Coral Reefs and Islands.

The picturesque beauty of the coral atoll, seated 'mid a waste of troubled waters, with its circlet of living green, its quiet, placid lagoon, and its marvellous submarine zoological gardens, has long been celebrated in the descriptions of voyagers to tropical seas. The attempt to arrive at a correct explanation of the general and characteristic form and features of these reefs and islands has, for an equally long period of time, exercised the ingenuity of thoughtful men.

Coral reefs are the most gigantic and remarkable organic accumulations on the face of the earth. They are met with in certain tropical regions, and are huge masses of carbonate of lime, secreted from ocean waters by myriads of marine organisms. While the great bulk of the reef consists of dead corals, skeletons and shells, the outer surface is clothed with a living mantle of plants and animals. This is especially the case on the outer and seaward face of the reef, where there are, at all times, myriads upon myriads of outstretched and hungry mouths, and not the least interesting questions connected with a coral reef are those relating to how these hungry mouths are satisfied.

It is to the power of these organisms of secreting carbonate of lime from sea water—building up and out generation after generation on their dead selves—that the coral reef owes its origin. So wonderful and unique is the result, that combination for a definite end has sometimes been attributed to these reef-builders.

There is, however, another process ever at work in the ocean, in a sense antagonistic to that of secretion of carbonate of lime by organisms, which has much to do in fashioning the more characteristic features of coral reefs. This is the solution of all dead carbonate of lime shells, skeletons, and calcareous debris, wherever these are exposed to the action of sea water. As soon as life loses its hold on the coral structures, and wherever these dead carbonate of lime remains are unprotected by rapid accumulation, they are silently, surely, and steadily removed in solution. This appears to be one of the best established oceanographical facts, and any theories concerning the general economy of the ocean which fail to take account of

this universal agency are most likely to be at fault. We know something about the rate of solution, probably more than we do about the rate of growth and secretion of carbonate of lime by the coral polyps. It has been shown that the rate of solution varies with temperature, with pressure, and with the amount of carbonic acid present in the water. It is on the play of these two opposing forces—the one vital and the other chemical—and their varying activity in different regions and under different circumstances, that we rely for the explanation of many oceanographical phenomena, especially many of those connected with oceanic deposits and coral reefs. In some regions there may be more growth, secretion and deposition of shell and coral materials than solution by sea water, and then there results the formation of coral reefs and vast calcareous deposits at the bottom of the ocean. There may be an almost exact balance between these processes. And again, there may be more solution than secretion, as, for instance, in the red clay areas, which occupy the deepest parts of the ocean, and in some coral reef lagoons.

What is the nature of the foundations of these coral islands, surrounded as they sometimes are by an ocean miles in depth? Why have some elongated reefs no lagoons? Why have most of the lagoons of the smaller atolls been filled up? Why is the circle of land or reef in the perfect atolls only, at most, a few hundred yards in diameter? What is the origin of the lagoon? What relation exists between the depth of the lagoon, its area, and the depth of the water beyond the outer reef? How has the dry land of these islands been formed, provided with a soil, a fauna and a flora? These appear to be the chief questions that demand an answer from any theory of coral island formation.

These coral formations are essentially structures belonging to the great oceans and ocean basins. They are dots of land within the oceanic areas that might be compared or contrasted with the small salt lakes which are scattered over the surface of the continental lands. A rapid survey of some of the more general phenomena of the great oceans may, then, lead to a better appreciation of the problems connected with coral reefs.

The great ocean basins occupy over two-thirds of the earth's surface, and have a mean depth of over two miles. The central portions of these basins, called the abysmal regions, occupy about one-half of the earth's surface, and have a mean depression below the general level of the continents of over three miles. The abysmal regions are vast undulating plains, sometimes rising to less than two miles from the surface of the sea, and again sinking to four and five miles beneath it. Volcanic cones rise singly or in clusters from these great submerged plains. When they shoot above the level of the sea they form single islands, like Ascension and St. Paul's Rocks, or groups, like the Azores, the Sandwich, the Fiji, and the Society Islands. As might have been expected, there are many more of these cones hidden beneath the waves than rise above them. When the

Challenger sounded along the west coast of Africa, there was no suspicion that between her stations she was sailing over submerged cones. Since then, however, the soundings of telegraph ships have correctly mapped out no less than seven of these peaks between the latitude of Lisbon and the island of Teneriffe. The depths on the summits of these vary from 12 to 500 fathoms. On one of them, at 400 fathoms, two species of coral (*Lophohelia prolifera* and *Amphihelia oculata*) were growing luxuriantly. Throughout the ocean basins about 300 such submarine cones, rising from great depths up to within depths of from 500 to 10 fathoms from the surface, are already known, or indicated by soundings.

All the agencies at work above the lower limit of wave action tend to wear away and level down these cones, and thus to form banks. Graham's Island, thrown up in the Mediterranean in 1831, was 200 feet in height and three miles in circumference, and was washed away in a year or two. The bank left on the spot, at first very shallow, has now 24 feet of water over it. Instances similar to this historical example must often have happened in the great ocean basins. Again, the same agencies produce wide banks around volcanic islands by washing away and spreading out the materials of the softer rocks. Such banks, with depths of less than 60 fathoms, are found extending many miles seawards around some volcanic islands.

On the other hand, all the deeply submerged summits are continually being built up to the lower limit of wave action by the accumulation of the remains of animals which live on them and by the fall of shells upon them from the surface waters. In the Solomon Islands, Dr. Brougham Guppy has shown that there are upraised coral islands with central volcanic cones covered with thick layers of marine deposits; Christmas Island in the Indian Ocean is another instance, and similar deposits must now be forming over hundreds of submerged mountains. In this way are foundations prepared for the true reef-building species, which only flourish in the shallower depths.

The bulk of the water of the ocean has a very low temperature: it is ice-cold at the bottom, even under the equator, but on the surface within the tropics, there is a relatively thin film of warm water, with a temperature of from 70° to 84° Fahr. This film of warm water is much deeper towards the western parts of the Atlantic and Pacific than it is in the eastern, the reason for this being that the trade winds, which blow continually from the east, carry all the warm surface water to the westward, and draw up cold water from beneath along the western shores of Africa and America to supply the place of that driven westward at the surface. Consequently, there is, at times, a very low temperature, and a great annual range of temperature, along these western shores. This is more clearly shown by the temperatures at 50 and 100 fathoms than by those at the surface. There are no coral reefs along the western shores of Africa and

South America, a circumstance evidently connected with the low temperature, wide range, and, more directly, with the food supply, consequent on these conditions. It appears to be a confirmation of this view that, on the eastern shores of Africa, about Cape Gardafui, from off which the south-west monsoon blows for several months in the year, cold water is also drawn to the surface, and there, likewise, are no coral reefs, though they flourish to the north and south of this region.

Coral reefs flourish in mid-ocean and along the eastern shores of the continents, or wherever the coasts are bathed by the warmest and purest currents of water coming directly from the open sea. If we except Bermuda and one or two other outlying reefs, where the temperature may occasionally fall to 66° Fahr. or 64° Fahr., it may be said that reefs are never found where the surface temperature of the water, at any time of the year, sinks below 70° Fahr., and where the annual range is greater than 12° Fahr. In typical coral reef regions, however, the temperature is higher and the range much less.

The food supply of the coral reef is derived from pelagic oceanic organisms, which exist in the greatest variety and abundance in the surface and sub-surface waters of the ocean. These consist of myriads of algæ, rhizopods, infusorians, medusæ, annelids, molluscs, crustaceans, ascidians, and fishes. A very large number of these creatures, within the tropics, secrete carbonate of lime from the ocean to form their shells and skeletons, which, falling to the bottom after death, form the vast oceanic deposits known as Pteropod and Globigerina oozes. In falling to the bottom, they carry down some of the organic matter that composed their living bodies, and thus are the animals which live on the floor of the ocean chiefly supplied with food. Here it may be remarked that the abundance of life at depths of even over two miles is very great. Our small dredges sometimes bring up over sixty species and hundreds of specimens in one haul—of invertebrates and fishes, exclusive of the protozoa. These pelagic organisms oscillate from the surface down to about 80 or 100 fathoms, probably that stratum of the ocean affected by sunlight, and they apparently descend further in regions where the stratum of warm water has a greater depth. Many of the forms rise to the surface in the evening and during calms, and sink again in sunlight and during stormy weather. It is in the evening and when it is calm that this swarming life is most vividly forced on the attention by gorgeous phosphorescent displays. The lime-secreting organisms, like Coccospheres and Rhabdospheres, Foraminifera, Pteropods, and other Molluscs, are much more abundant, both in species and individuals, in the warmest and saltest waters than elsewhere. I have estimated, from tow-net experiments, that at least 16 tons of carbonate of lime, in the form of these shells, exist in a mass of the ocean, in coral reef regions, one mile square by 100 fathoms in depth. If we take this estimate, which I consider much below the reality, and suppose

one-sixteenth of these organisms to die and fall to the bottom each day, then they would take between 400 and 500 years to form a deposit one inch in thickness. I give this calculation more to indicate a method than to give even the roughest approximation to a rate of accumulation of deposits. The experiments were too few to warrant any definite deductions.

The great oceanic currents moving westward at the rate of several miles an hour, bear these shoals of pelagic organisms on to the face of the reef, where millions of greedy mouths are ready and eager to receive them. The corals and other organisms situated on the outer and windward side of the reef receive the first and best supply; they are thus endowed with a greater amount of energy, and grow faster and more luxuriantly there than on other portions of the reef; the depth at which there is the most constant supply of this food is several fathoms beneath the surface, and there too the corals are found in most vigorous growth. It is only a relatively small quantity of this pelagic food that enters the lagoon, the corals that there struggle on in patches being largely supplied with the means of existence from the larvæ of reef-building animals.

So many observations were made during the *Challenger* Expedition on the pelagic fauna inside and outside reefs that there is little, if any, doubt in my mind that the food supply is a most important factor in relation to the growth of corals in the different portions of a reef. Actual observations were made on the feeding of corals at a good many places, as well as numerous observations on the stomach contents. These observations have been confirmed by Alexander Agassiz.

It is not possible to state in what form the carbonate of lime that is secreted in such enormous quantities by marine organisms exists in oceanic waters.

The following table shows the average composition of sea-water salts, the acids and bases being combined in the way usually adopted by chemists.

Average Composition of Sea Salt.

Chloride of sodium	77·758
Chloride of magnesium	10·878
Sulphate of magnesium	4·737
Sulphate of lime	3·600
Sulphate of potash	2·465
Bromide of magnesium	0·217
Carbonate of lime	0·345
	100·000

In the actual ocean water there are probably traces of every known element, and it is impossible to say what is the precise amount of the respective chlorides, sulphates, and carbonates present. Theoretically, every base may be combined with every acid, and the whole solution

must be in a continual state of flux as to its internal composition. While the quantity of sea salts in a given volume of water varies with position, yet it has been shown by hundreds of analyses that the actual ratio of acids and bases—that is, the ratio of the constituents of sea salts—is constant in waters from all regions and depths, with one very significant exception—that of lime—which is present in slightly greater proportion in deep water.

The total amount of *calcium* in a cubic mile of sea water is estimated at nearly 2,000,000 tons. The amount of the same element present in a cubic mile of river water is nearly 150,000 tons. At the rate at which rivers carry down water from the land it is estimated that it would take 680,000 years to pour into the ocean an amount of calcium equal to that now held by the ocean in solution.

The amount of calcium existing in the 40,000,000 square miles of the typical calcareous deposits of the ocean exceeds, however, that at present held in solution if we merely take them to have an average thickness of 30 feet, and from this calculation we might say that, if the secretion and solution of lime in the other regions of the ocean be exactly balanced, and the calcium in the ocean remain always constant, those calcareous deposits of the thickness indicated would require between 600,000 and 700,000 years to accumulate. There is good evidence, however, that the rate of accumulation is much more rapid in some positions.

The lime thus carried down to the sea is originally derived from the decomposition of anhydrous minerals, and comes from the land in the form of carbonate, phosphate, and sulphate of lime—the carbonate being in the greatest abundance in river water. On the other hand, the sulphate of lime very greatly predominates in sea water, the carbonates being present in small quantity. We are not in a position to say whether or not the coral polyps take the whole of the material for their skeletons from the carbonates, as is generally believed, or indeed to say what changes take place during the progress of secretion by organisms.

In the greatest depths of the Pacific coral seas there is striking evidence of the solvent power of ocean water. Our dredges bring up from a depth of three or four miles over a hundred ear-bones of whales and remnants of the dense Ziphioid beaks, but all the larger and more areolar bones of these immense animals have been almost entirely removed by solution. In a single haul there may also be many hundreds of sharks' teeth, some of them larger than the fossil *Carcharodon* teeth, but all that remains of them is the hard dentine. None of the numerous calcareous surface shells reach the bottom, although they are quite as abundant over the red clay areas as over those shallower areas where they form Globigerina and Pteropod deposits. In consequence of the small amount of detrital material reaching these abysmal areas distant from continents, cosmic metallic spherules, manganese nodules, highly altered volcanic fragments, and zeolitic

minerals, are there found in great numbers. Almost all these things are found occasionally in the other regions of the ocean's bed, but their presence is generally masked by the accumulation of other matters. In some regions Radiolarian and Diatom remains are found in the greatest depths, and they too are subject to the solvent power of sea water, but to a much less extent than carbonate of lime shells.

As we ascend to shallower waters, a few of the thicker shelled specimens are met with at first, with lesser depths the carbonate of lime shells increase in number, until in the shallower deposits the remains of Pteropods, Heteropods, and the most delicate larval shells are present in the deposit at the bottom. This gradation in the appearance of the shells can be well seen in a series of soundings at different depths around a volcanic cone, such as has been described as forming the base of a coral atoll. There is no known way of accounting for this vertical distribution of these dead shells except by admitting that they have been dissolved away in sinking through the deeper strata of water, or shortly after reaching the bottom; indeed, an examination of the shells themselves almost shows the process in operation. It is rare to find any trace of fish bones in deposits other than the otoliths.

These considerations, as well as numerous experiments in the laboratory, show that everywhere in the ocean dead carbonate of lime structures slowly disappear wherever they are exposed to the action of sea water, and in investigating the evolution of the general features of coral reefs it is as necessary to take cognisance of this fact as of the secretion of carbonate of lime by organisms.

The first stage, then, in the history of a coral island is the preparation of a suitable foundation on submerged volcanic cones, or along the shores of a volcanic island, or the borders of a continent. In the case of the atoll the cone may have been reduced below the level of the sea by the waves and atmospheric influences, or built up to the lower limit of breaker action by the vast accumulation of organisms on its summit.

A time comes, however, should the peak be situated in a region where the temperature is sufficiently high, and the surface currents contain a suitable quality of food, that the reef-builders fix themselves on the bank. The massive structure which they secrete from ocean water enables them to build up and maintain their position in the very face of ocean currents, of breakers, of the overwhelming and outrageous sea.*

"Coral" with the sailor or marine surveyor is usually any carbonate

* Dr. Brougham Guppy says, "History can afford us no clue to the first appearance or the age of reefs; yet in the myths of the Pacific Islanders we find that the savage inhabitants of these regions regard the history of a coral atoll as commencing with the submerged shoal, which through the agency of God-like heroes is brought up by their fish-hooks to the surface."—*Paper, Vict. Inst.*

of lime shell or skeleton or their broken-down parts. "Coral" is used by the naturalist in a much more restricted sense; he limits the term to animals classed as Madrepores, Hydrocorallines, and Alcyonarians. The animals belonging to the first two of these orders comprise those included under the vague term of reef corals. Besides these, however, very many other classes of animals contribute to the building up of coral reefs and islands—such are Foraminifera, Sponges, Polyzoa, Annelids, Echinoderms, and Calcareous Algæ. The relative proportions of these different organisms in a reef vary with the region, with the depth, and with the temperature, but members of what are known under the term of reef corals appear always to predominate.

The animals of the true reef-building species resemble the common sea anemones in structure and size; the individual polyps may vary from the eighth of an inch in diameter to over a foot. Some of the structures built by colonies may exceed 20 feet in diameter.

There may be great variety in the appearance of submerged reefs as they rise from banks of a different nature, form, and extent, as, indeed, was pointed out long ago by Chamisso. There may be differences due also to the kinds and abundance of deep-sea animals living on such banks, as well as differences due to currents, temperature, and other meteorological conditions.

From the very first the plantations situated on the outer edge will have the advantage, from the more abundant supply of food and the absence of sand in the water, which last more or less injuriously affects those placed towards the interior. Chamisso attributed the existence of the lagoon to the more vigorous growth of the peripherally situated corals of a reef, as compared with those placed towards the middle, and in this he was to a large extent right, but the symmetrical form of the completed atoll is chiefly due to the solution of the dead carbonate of lime structures. The Great Chagos Bank illustrates the irregular way in which such a large bank of coral plantations approaches the surface. When these, however, reach the surface, they assume slowly a more regular outline, those on the outer edge coalesce, and ultimately form a complete ring of coral reef, and the lagoon becomes gradually cleared of its coral patches or islands, for as the atoll becomes more perfect, the conditions of life within the lagoon become less and less favourable, and a larger quantity of dead coral is removed in solution.

The coral atoll varies greatly in size and form: it is usually more or less circular, and may be one or over fifty miles in diameter. The breakers spend their fury on the outer edge, and produce what is known as the broad shore platform; but within, trees descend to the very shore of the lagoon, where there is quiet water, and a ship may often enter on the lee side of the atoll and find safe anchorage.

In this connection it is important to bear in mind the relation which exists between the periphery and the superficial area of the lagoon in atolls of different sizes. If the coral plantations which rise

from the top of a submerged mountain have an area of one square mile, then on reaching the surface of the waves there will be a shallow depression in the centre owing to the more rapid growth of the outer edge. Such an atoll will have, if it be a square, four miles of outer reef for the supply of coral sand and other debris, and these being washed and blown into the one square mile of shallow lagoon it is likely to become filled up, the result being a small island with dry lagoon in which may be found deposits of sulphate of lime, magnesian and phosphatic rocks, and guano—all these testifying to the great age of the island and absence of subsidence in the region. It is only atolls with a diameter of less than two miles that thus become filled up. In other and larger plantations, rising from a more extensive bank, the conditions are very different. In this larger atoll—say four miles square—there is now only one mile of outer reef to each square mile of lagoon, instead of four miles of outer reef to the one square mile of lagoon in the smaller atoll. Only one-fourth of the detrital matter and food enters the larger lagoon, from the outside, per square mile of lagoon, and hence there is proportionally less living coral; the solvent agencies predominate and the lagoon is widened and deepened. Growing seawards on the outer face and dissolving away in the lagoon, the whole expands after the manner of a fairy ring, and the ribbon of reef or land can never in consequence increase beyond a half or three-quarters of a mile in width, it being usually much less.

Atolls may occur far away from any other land, but it more frequently happens that they are arranged in linear groups, in this respect resembling volcanic islands. Extensive banks may be crowded with small atolls, like the Northern Maldives; or a bank may be occupied by one great and perfect atoll 20 to 40 miles in diameter, like some of the Southern Maldives and the Paumotus. In some instances the large atolls appear to have resulted from the growth and coalescence of the smaller marginal atolls; especially does this seem to have been the case with the large Southern Maldives.

The outer slopes vary greatly in different reefs, and in different parts of the same reef. When there is deep water beyond, the reef very often extends out with a gentle slope to a depth of 25 to 40 fathoms, and is studded with living coral, the bosses and knobs becoming larger in the deeper water farthest from the reef, where there are great overhanging cliffs, which eventually fall away by their own weight and form a talus on which the reef may proceed further outwards. Occasionally there is a very steep descent almost at once from the outer edge. Thus the deeper the water beyond, the more slowly will the reef extend seawards. In reefs with a very gentle slope outside, the corals are frequently overhanging at depths of six or seven fathoms, for in these instances the lower part of the sea-face of the reef is rendered unsuitable for vigorous growth, in consequence of the sand which is carried in by waves coming over the comparatively shallow depths outside.

As has been stated, the lagoon in many of the smallest atolls has been filled up, but this never appears to happen in atolls with a diameter of over two miles unless there be distinct evidence of upheaval. In perfectly formed atolls, that is, those in which the reefs are nearly continuous throughout, the deepest water is found towards the centre of the lagoon, and there is a relation between this depth and the depth of water beyond the outside reefs. In North and South Minerva reefs, in the South Pacific, where the outside depths are very great, there are depths down to 17 fathoms in the lagoons, which are apparently clear of coral heads. Here we may suppose that the central parts of the lagoon have for a long time been exposed to the solvent action of sea-water, owing to the slow lateral growth of the reef as a whole. In the same regions the Elizabeth and Middleton reefs, which are about the same size, have only four or five fathoms within the lagoons, and the depths outside the reefs are at the distance of a mile mostly within the 100 fathom line, and sometimes less than 50 fathoms. There are also many coral heads within the lagoons. Here we may suppose the atolls to be more recent and to have extended more rapidly than in the case of the Minerva reefs. If the depths beyond the reefs be taken into consideration, then there is usually a direct relation between the depth of the lagoon and its diameter. The greatest depths, even in the largest atolls, do not exceed 50, or at most 60, fathoms; they are usually much less. In atolls which are deeply submerged, or have not yet reached the surface, which have wide and deep openings into the lagoon-like spaces, this relation may not exist. In these instances the secretion and deposition of carbonate of lime may be in excess of solution in all parts of the lagoon. It is only when the atoll reaches the surface, becomes more perfect, and its lagoon waters consequently less favourable to growth, that the solution of the dead corals and calcareous débris exceeds any secretion and deposition that may take place throughout the whole extent of the lagoon; it is then widened and deepened, and formed into a more or less perfect cup-like depression.

The whole of a coral reef is permeated with sea-water like a sponge; as this water is but slowly changed in the interior parts it becomes saturated, and a deposition of crystalline carbonate of lime frequently takes place among the interstices of the corals and coral débris. In consequence of the solution of coral débris and the re-deposited lime occupying less space, large cavities are formed, and this process often results in local depression in some islands, as, for instance, in Bermuda. At many points on a reef where evaporation takes place there is a deposition of amorphous carbonate of lime cementing the whole reef materials into a compact conglomerate-like rock.

The fragments of the various organisms broken off from the outer edge during gales or storms are piled up on the upper surface of the reef, and eventually ground into sand, the result being the formation

of a sandy cay or shoal at some distance back from the outer edge of the reef—the first stage in the formation of dry land.

The fragments of pumice thrown up into the ocean during far-distant submarine eruptions, or washed down from volcanic lands, are at all times to be found floating about on the surface of the sea, and these being cast upon the newly formed islet produce by their disintegration the clayey materials for the formation of a soil—the red earth of coral islands. Just within the shore platform these pumice fragments are found in a fresh condition, but as the lagoon is approached they disappear, the soil becomes deeper, and the most luxuriant vegetation and largest trees are found close to the edge of the inner waters. The land is seldom continuous around the atoll; it occurs usually in patches. The water passes over the shallow spaces between the islets and through the deeper lagoon entrances, these last being kept open by the strong sand-bearing currents which pass at each tide.

The few species of plants and animals which inhabit these coral islands have been drifted to the new island like the pumice, or carried, many of them may be, by birds; lastly, savage and civilised man finds there a home.

There is no essential difference between the reefs forming fringing and barrier reefs, and those which are known as atolls. In the former case the corals have commenced to grow close to the shore, and as they grow outward, a small boat passage, and then a ship channel, is carved out between the reef and the shore by tidal scour and the solvent action of the water on the dead parts of the reef: thus the fringing reef may be converted into a barrier reef. In some instances the corals find a suitable foundation on the banks that surround islands and front continental lands, it may be, at a great distance from the coast, and when they reach the surface they form a distant barrier which proceeds seawards, ultimately on a talus made up of materials torn from its seaward face.

If the foregoing considerations be just and tenable, then it would appear that all the characteristic features of coral reefs can be produced, alike in stationary areas or in areas of slow elevation and subsidence, by processes continually at work in the ocean at the present time. Slow elevation or subsidence would only modify in a minor way a typical coral atoll or barrier reef, but subsidence in past times cannot be regarded as the cause of the leading characteristics of coral reefs. There are abundant evidences of elevation in coral reef regions in recent times, but no direct evidence of subsidence. If it has been shown that atoll and barrier reefs can be formed without subsidence, then it is most unlikely that their presence in any way indicates regions of the earth's surface where there have been wide, general, and slow depressions.

According to Mr. Darwin's theory, which has been almost universally accepted during the past half century, the corals commence to grow close to the shore of an island or continent: as the land

slowly sinks the corals meanwhile grow upwards to the surface of the sea, and a water space—the lagoon channel—is formed between the shore of the island and the encircling reef, the fringing being thus converted into a barrier reef. Eventually the central island sinks altogether from sight and the barrier reef is converted into an atoll, the lagoon marking the place where the volcanic or other land once existed. Encircling reefs and atolls are represented as becoming smaller and smaller as the sinking goes on, and the final stage of the atoll is a small coral islet, less than two miles in diameter, with the lagoon filled up and covered with deposits of sea-salts and guano.

It is at once evident that the views now advocated are in almost all respects the reverse of those demanded by Mr. Darwin's theory.

The recent deep-sea investigations do not appear in any way to support the view that large or small islands once filled the spaces now occupied by the lagoon waters, and that the reefs show approximately the position of the shores of a subsided island. The structure of the upraised coral islands, so far as yet examined, appears to lend no support to the Darwinian theory of formation. When we remember that the great growing surface of existing reefs is the seaward face from the sea surface down to twenty or forty fathoms, that large quantities of coral débris must be annually removed from lagoons in suspension and solution, that reefs expand laterally and remain always but a few hundred yards in width, that the lagoons of finished atolls are deepest in the centre, and are relatively shallow compared with the depth off the outer reefs, then it seems impossible with our present knowledge to admit that atolls or barrier reefs have ever been developed after the manner indicated by Mr. Darwin's simple and beautiful theory of coral reefs.

[J. M.]

WEEKLY EVENING MEETING,

Friday, March 23, 1888.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

Sir Frederick Bramwell, D.C.L. F.R.S. *Hon. Sec. and V.P. R.I.*

A Lecture with,—and without,—point.

[The discourse mainly consisted of a demonstration of the serious inconveniences attending the adoption in ordinary life of the Decimal and Metrical Systems.]

(Abstract deferred.)

GENERAL MONTHLY MEETING,

Monday, April 2, 1888.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Manager, in the Chair.

William Arthur Brailey, M.D. M.A. M.R.C.S.
Thomas R. Dallmeyer, Esq.
Samuel de Lissa, Esq.
Walter Gilbey, Esq.
Colonel Alexander Charles Hamilton, R.E.
Robert Moon, Esq.
Fitzpatrick Praed, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to His Grace the Duke of Northumberland, K.G. the President, for his present of 'Annals of the House of Percy (1030–1887). By E. B. de Fonblanque, 2 vol. 1887'; and to Lieutenant-General Pitt-Rivers, *M.R.I.* for his present of 'Excavations in Cranborne Chase, near Rushmore. Vol. I. 1887.'

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Report of a Tour in the Panjâb and Râjpûtâna in 1883–4. By H. B. W. Garrick (Archæol. Survey of India). 8vo. 1887.
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Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. III. 2° Semestre, Fasc. 10, 11. 8vo. 1887.
Académie des Sciences de l'Institut, Paris—Mémoires, Tome XLII. 4to. 1883.
Mémoires présentés par Divers Savants, Tomes XXVII. XXVIII. XXIX. 4to. 1883–7.
Receuil de Mémoires, etc. relatifs à l'Observation du Passage de Vénus sur le Soleil, Tome II. Partie 2; Tome III. Parties 1–3. 4to. 1880–5.
American Philosophical Society—Proceedings, No. 126. 8vo. 1887.
Asiatic Society, Royal (*China Branch*)—Journal, Vol. XXII. Nos. 1, 2. 8vo. 1887.
Astronomical Society, Royal—Monthly Notices, Vol. XLVIII. No. 4. 8vo. 1888.
Memoirs, Vol. XLIX. Part 1. 4to. 1888.
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Rainfall in East Indian Archipelago, 1886. 8vo. 1887.

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Chazarain, Dr. (*the Author*)—Les Courants de la Polarité dans l'Aimant et dans le Corps Humain. 8vo. 1887.
Chemical Society—Journal for March 1888. 8vo.
Churchill, Messrs. J. and A. (*the Publishers*) — Journal of Laryngology and Rhinology, Vol. II. Nos. 1, 2, 3. 8vo. 1888.
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Analyst for March, 1888. 8vo.
Athenæum for March, 1888. 4to.
Chemical News for March, 1888. 4to.
Chemist and Druggist for March, 1888. 8vo.
Engineer for March, 1888. fol.
Engineering for March, 1888. fol.
Horological Journal for March, 1888. 8vo.
Industries for March, 1888. fol.
Iron for March, 1888. 4to.
Murray's Magazine for March, 1888. 8vo.
Nature for March, 1888. 4to.
Revue Scientifique for March, 1888. 4to.
Scientific News for March, 1888. 4to.
Telegraphic Journal for March, 1888. 8vo.
Zoophilist for March, 1888. 4to.
Foster, J. Esq. (*the Editor*)—Alumni Oxonienses: The Members of the University of Oxford, 1715–1886. Vol. I. 8vo. 1888.
Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 52, 53. 8vo. 1888.
Franklin Institute—Journal, No. 747. 8vo. 1888.
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Supplementary Papers, Vol. II. Part 2. 8vo. 1888.
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Studies in Historical and Political Science, Fifth Series, No. 12. 8vo. 1887.
University Circular, No. 63. 4to. 1888.
Linnean Society—Journal, No. 162. 8vo. 1888.
Manchester Geological Society—Transactions, Vol. XIX. Parts 14, 15. 8vo. 1888.
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Proceedings, Vol. XXV. XXVI. 8vo. 1885–7.
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Meteorological Office—Weekly Weather Reports, Vol. IV. Nos. 46–52. Appendix 1–4. Vol. V. Nos. 1–7. 4to. 1887–8.
Hourly Readings, 1885, Part 2. 4to. 1888.
Monthly Weather Reports, Jan. Feb. 1887. 4to.
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Bulletin. Nouvelle Serie, No. 4. 4to. Toulouse, 1887.
Society for the Abolition of Vivisection—The Brown Animal Sanatory Institution. 8vo. 1888.
Society of Arts—Journal, March, 1888. 8vo.
St. Bartholomew's Hospital—Reports, Vol. XXIII. 8vo. 1887.
St. Pétersbourg Académie Impériales des Sciences—Mémoires, Tome XXXV. No. 10. 4to. 1887.
Telegraph Engineers, Society of—Journal, No. 70. 8vo. 1888.
The British Museum (Natural History)—Catalogue of Birds, Vol. XII. 8vo. 1888.
Catalogue of Fossil Mammalia, Part 5. 8vo. 1887.
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Tomalin, Lewis R. S. Esq. (the Author)—Essays on Health Culture. By G. Jaeger. 8vo. 1887.
Vereins zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1888: Heft, 2. 4to.
Velschow, Franz A. Esq. C. E. (the Author)—The Natural Law of relation between Rainfall and Vegetable Life. 8vo. 1888.

WEEKLY EVENING MEETING.

Friday, April 13, 1888.

Edward Woods, Esq. M.Inst.C.E. Vice-President, in the Chair.

Professor William Henry Flower, C.B. LL.D. F.R.S.

The Pygmy Races of Men.

It is well known that the nations of antiquity entertained a widespread belief in the existence of a race or races of human beings of exceedingly diminutive stature, who dwelt in some of the remote and unexplored regions of the earth. These were called *Pygmies*, a word said to be derived from *πυγμή*, which means a fist, and also a measure of length, the distance from the elbow to the knuckles of an ordinary-sized man, or rather more than 13 inches.

In the opening of the third book of the *Iliad*, the Trojan hosts are described as coming on with noise and shouting, "like the cranes which flee from the coming of winter and sudden rain, and fly with clamour towards the streams of ocean, bearing slaughter and fate to the Pygmy men, and in early morn offer cruel battle," or, as Pope has it—

> "So when inclement winters vex the plain,
> With piercing frosts, or thick descending rain,
> To warmer seas the cranes embodied fly,
> With noise and order through the midway sky,
> To Pygmy nations wounds and death they bring,
> And all the war descends upon the wing."

The combats between the pygmies and the cranes are often alluded to by later classical writers, and are not unfrequently depicted upon Greek vases. In one of these in the Hope collection at Deepdene, in which the figures are represented with great spirit, the pygmies are dwarfish-looking men with large heads, negro features, and close woolly or frizzly hair. They are armed with lances. Notices of a less poetical and apparently more scientific character of the occurrence of races of very small human beings are met with in Aristotle, Herodotus, Ctesias, Pliny, Pomponius Melo, and others. Aristotle places his pygmies in Africa, near the sources of the Nile, while Ctesias describes a race of dwarfs in the interior of India. The account in Herodotus is so circumstantial, and has such an air of truthfulness about it, especially in connection with recent discoveries, that it is worth quoting in full.*

"I did hear, indeed, what I will now relate, from certain natives of Cyrêné. Once upon a time, they said, they were on a visit to the

* Herodotus, Book II., 32, Rawlinson's translation, p. 47.

oracular shrine of Ammon, when it chanced that, in the course of conversation with Etearchus, the Ammonian king, the talk fell upon the Nile, how that its sources were unknown to all men. Etearchus upon this mentioned that some Nasamonians had once come to his court, and when asked if they could give any information concerning the uninhabited parts of Libya, had told the following tale. (The Nasamonians are a Libyan race who occupy the Syrtes, and a tract of no great size towards the east.) They said there had grown up among them some wild young men, the sons of certain chiefs, who, when they came to man's estate, indulged in all manner of extravagancies, and among other things drew lots for five of their number to go and explore the desert parts of Libya, and try if they could not penetrate further than any had done previously. The young men therefore dispatched on this errand by their comrades with a plentiful supply of water and provisions, travelled at first through the inhabited region, passing which they came to the wild beast tract, whence they finally entered upon the desert, which they proceeded to cross in a direction from east to west. After journeying for many days over a wide extent of sand, they came at last to a plain where they observed trees growing: approaching them, and seeing fruit on them, they proceeded to gather it. While they were thus engaged, there came upon them some dwarfish men, under the middle height, who seized them and carried them off. The Nasamonians could not understand a word of their language, nor had they any acquaintance with the language of the Nasamonians. They were led across extensive marshes, and finally came to a town, where all the men were of the height of their conductors, and black-complexioned. A great river flowed by the town, running from west to east, and containing crocodiles."

It is satisfactory to know that the narrative concludes by saying that these pioneers of African exploration, forerunners of Bruce and Park, of Barth, Livingstone, Speke, Grant, Schweinfurth, Stanley, and the rest, "got safe back to their country."

Extension of knowledge of the natural products of the earth, and a more critical spirit on the part of authors, led to attempts to account for this belief, and the discovery of races of monkeys—of the doings of which, it must be said, more or less fabulous stories were often reported by travellers—generally sufficed the commentators and naturalists of the last century to explain the origin of the stories of the pygmies. To this view the great authority of Buffon was extended.

Still more recently-acquired information as to the actual condition of the human population of the globe has, however, led to a revision of the ideas upon the subject, and to more careful and critical researches into the ancient documents. M. de Quatrefages, the eminent and veteran Professor of Anthropology at the Muséum d'Histoire Naturelle of Paris, especially, has carefully examined and collated all the evidence bearing upon the question, and devoted much ingenuity of argument to prove that the two localities in which the ancient authors appear to place their pygmies, the interior of Africa near

the sources of the Nile, and the southernmost parts of Asia, and the characters they assign to them, indicate an actual knowledge of the existence of the two groups of small people which still inhabit these regions, the history of which will form the subject of this lecture. The evidence which has convinced M. de Quatrefages, and which, I have no doubt, will suffice for those who take pleasure in discovering an underlying truth in all such legends and myths, or in the more grateful task of rehabilitating the veracity of the fathers of literature and history, will be found collected in a very readable form in a little book published last year in the 'Bibliothèque scientifique contemporaine,' called 'Les Pygmées,' to which I refer my hearers for fuller information upon the subject of this discourse, and especially for numerous references to the literature of the subject, which, as the book is accessible to all who wish to pursue it further, I need not give here.

It is still, however, to my mind, an open question whether these old stories may not be classed with innumerable others, the offspring of the fertile invention of the human brain, the potency of which as an origin of myths has, I think, sometimes been too much underrated. I shall, therefore, now take leave of them, and confine myself to giving you, as far as the brief space of time at my disposal admits, an account of our actual knowledge of the smallest races of men either existing, or, as far as we know, ever having existed on earth, and which may, therefore, taking the word in its current though not literal sense, be called the "pygmies" of the species.

Among the various characters by which the different races of men are distinguished from one another, *size* is undoubtedly one of considerable importance. Not but what in each race there is much individual variation, some persons being taller and some shorter; yet these variations are, especially in the purer or less mixed races, restricted within certain limits, and there is a general average, both for men and women, which can be ascertained when a sufficient number of accurate measurements have been recorded. That the prevailing size of a race is a really deeply-seated, inherited characteristic, and depends but little on outward conditions, as abundance of food, climate, &c., is proved by well-known facts. The tallest and the shortest races in Europe are respectively the Norwegians and the Lapps, living in almost the same region. In Africa, also, the diminutive Bushmen and the tallest race of the country, the Kaffirs, are close neighbours. The natives of the Andaman Islands and those of many islands of the equatorial region of the Pacific, in which the conditions are similar, or if anything more favourable to the former, are at opposite ends of the scale of height. Those not accustomed to the difficulties both of making and recording such measurements will scarcely be prepared, however, to learn how meagre, unsatisfactory, and unreliable our knowledge of the stature of most of the races of mankind is at present, although unquestionably it has been considerably increased within recent years. We must, however, make use of

such material as we possess, and trust to the future correction of errors when better opportunities occur.

It is convenient to divide men, according to their height, into three groups—tall, medium, and short; in Topinard's system,* the first being those the average height (of the men) of which is above 1·700 metres (5 feet 7 inches), the last those below 1·600 metres (5 feet 3 inches), and the middle division those between the two. In the short division are included certain of the Mongolian or yellow races of Asia, as the Samoyedes, the Ostiaks, the Japanese, the Siamese, and the Annamites; also the Veddahs of Ceylon and certain of the wild hill-tribes of Southern India. These all range between 1·525 and 1·600 metres—say between 5 feet and 5 feet 3 inches.

It is of none of these people that I am going to speak to-day. My pygmies are all on a still smaller scale, the average height of the men being in all cases below 5 feet, in some cases, as we shall see, considerably below.

Besides their diminutive size, I may note at the outset that they all have in a strongly-marked degree the character of the hair distinguished as frizzly—i. e. growing in very fine, close curls, and flattened or elliptical in section, and therefore, whatever other structural differences they present, they all belong to the same primary branch of the human species as the African negro and the Melanesian of the Western Pacific.

I will first direct your attention to a group of islands in the Indian Ocean—the Andamans—where we shall find a race in many respects of the greatest possible interest to the anthropologist.

These islands are situated in the Bay of Bengal between the 10th and 14th parallels of north latitude, and near the meridian 93° east of Greenwich, and consist of the Great and Little Andamans. The former is about 140 miles long, and has a breadth nowhere exceeding 20 miles. It is divided by narrow channels into three, called respectively North, Middle, and South Andaman, and there are also various smaller islands belonging to the group. Little Andaman is a detached island lying about 28 miles to the south of the main group, about 27 miles in length and 10 to 18 in breadth.

Although these islands have been inhabited for a very great length of time by people whose state of culture and customs have undergone little or no change, as proved by the examination of the contents of the old kitchen-middens, or refuse heaps, found in many places in them, and although they lie so near the track of civilisation and commerce, the islands and their inhabitants were practically unknown to the world until so recently as the year 1858. It is true that their existence is mentioned by Arabic writers of the ninth century, and again by Marco Polo, and that in 1788 an attempt was made to establish a penal colony upon them by the East India Company, which was abandoned a few years after; but the bad repu-

* 'Eléments d'Anthropologie Générale.' Paris, 1885, p. 463.

tation the inhabitants had acquired for ferocious and inhospitable treatment of strangers brought by accident to their shores, caused them to be carefully avoided, and no permanent settlement or relations of anything like a friendly character, or likely to afford any useful information as to the character of the islands or the inhabitants, were established. It is fair to mention that this hostility to foreigners, which for long was one of the chief characteristics by which the Andamanese were known to the outer world, found much justification in the cruel experiences they suffered from the malpractices, especially kidnapping for slavery, of the Chinese and Malay traders who visited the islands in search of *bêche de mer* and edible birds'-nests. It is also to this characteristic that the inhabitants owe so much of their interest to us from a scientific point of view, for we have here the rare case of a population, confined to a very limited space, and isolated for hundreds, perhaps thousands, of years from all contact with external influence, their physical characters unmixed by crossing, and their culture, their beliefs, their language entirely their own.

In 1857, when the Sepoy mutiny called the attention of the Indian Government to the necessity of a habitation for their numerous convict prisoners, the Andaman Islands were again thought of for the purpose. A Commission, consisting of Dr. F. J. Mouat, Dr. G. Playfair, and Lieut. J. A. Heathcote, was sent to the islands to report upon their capabilities for such a purpose; and, acting upon its recommendations, early in the following year the islands were taken possession of in the name of the East India Company by Captain (now General) H. Man, and the British flag hoisted at Port Blair, near the southern end of Great Andaman, which thenceforth became the nucleus of the settlement of invaders, now numbering about 15,000 persons, of whom more than three-fourths are convict prisoners, the rest soldiers, police, and the usual accompaniments of a military station.

The effect of this inroad upon the unsophisticated native population, who, though spread over the whole area of the islands, were far less numerous, may easily be imagined. It is simply deterioration of character, moral and physical decay, and finally extinction. The newly-introduced habits of life, vices, and diseases, are spreading at a fearful rate, and with deadly effect. In this sad history there are, however, two redeeming features which distinguish our occupation of the Andamans from that of Tasmania, where a similar tragedy was played out during the present century. In the first place, the British Governors and residents appear from the first to have used every effort to obtain for the natives the most careful and considerate treatment, and to alleviate as much as possible the evils which they have unintentionally been the means of inflicting on them. Secondly, most careful records have been preserved of the physical characters, the social customs, the arts, manufactures, traditions, and language of the people while still in their primitive condition. For this most

important work, a work which, if not done, would have left a blank in the history of the world which could never have been replaced, we are indebted almost entirely to the scientific enthusiasm of one individual, Mr. Edward Horace Man, who most fortunately happened to be in a position (as Assistant Superintendent of the Islands, and specially in charge of the natives) which enabled him to obtain the required information with facilities which probably no one else could have had, and whose observations 'On the Aboriginal Inhabitants of the Andaman Islands,' published by the Anthropological Institute of Great Britain and Ireland, are most valuable, not only for the information they contain, but as correcting the numerous erroneous and misleading statements circulated regarding these people by previous and less well-informed or less critical authors.

The Arab writer of the ninth century previously alluded to, states that "their complexion is frightful, their hair frizzled, their countenance and eyes frightful, their feet very large, and almost a cubit in length, and they go quite naked," while Marco Polo (about 1285) says that "the people are no better than wild beasts, and I assure you all the men of this island of Angamanain have heads like dogs, and teeth and eyes likewise; in fact, in the face they are just like big mastiff dogs." These specimens of mediæval anthropology are almost rivalled by the descriptions of the customs and moral character of the same people published as recently as 1862, based chiefly on information obtained from one of the runaway sepoy convicts, and which represent them as among the lowest and most degraded of human beings.

The natives of the Andamans are divided into nine distinct tribes, each inhabiting its own district. Eight of these live upon the Great Andaman Islands, and one upon the hitherto almost unexplored Little Andaman. Although each of these tribes possess a distinct dialect, these are all traceable to the same source, and are all in the same stage of development. The observations that have been made hitherto relate mostly to the tribe inhabiting the south island, but it does not appear that there is any great variation either in physical characters or manners, customs, and culture among them.

With regard to the important character of size, we have more abundant and more accurate information than of most other races. Mr. Man gives the measurements of forty-eight men and forty-one women, making the average of the former 4 feet $10\frac{3}{4}$ inches, that of the latter 4 feet $7\frac{1}{4}$ inches, a difference therefore of $3\frac{1}{2}$ inches between the sexes. The tallest man was 5 feet $4\frac{1}{4}$ inches; the shortest 4 feet 6 inches. The tallest woman 4 feet $11\frac{1}{2}$ inches; the shortest 4 feet 4 inches. Measurements made upon the living subject are always liable to errors, but it is possible that in so large a series these will compensate each other, and that therefore the averages may be relied upon. My own observations, based upon the measurements of the bones alone of as many as twenty-nine skeletons, give smaller averages, viz. 4 feet $8\frac{1}{2}$ inches for the men, and 4 feet $6\frac{1}{2}$

inches for the women; but these, it must be recollected, are calculated from the length of the femur, upon a ratio which, though usually correct for Europeans, may not hold good in the case of other races.* The hair is fine, and very closely curled; woolly, as it is generally called, or, rather, frizzly, and elliptical in section, as in the negroes. The colour of the skin is very dark, although not absolutely black. The head is of roundish (brachycephalic) form, the cephalic index of the skull being about 82. The other cranial characters are fully described in the papers just referred to. The teeth are large, but the jaws are only slightly prognathous. The features possess little of the negro type; at all events, little of the most marked and coarser peculiarities of that type. The projecting jaws, the prominent thick lips, the broad and flattened nose of the genuine negro are so softened down in the Andamanese as scarcely to be recognised, and yet in the relative proportions of the limb-bones, especially in the shortness of the humerus compared with the forearm, and in the form of the pelvis, negro affinities are most strongly indicated.

In speaking of the culture of the Andamanese, of course I only refer to their condition before the introduction of European civilisation into the islands. They live in small villages or encampments, in dwellings of simple and rude construction, built only of branches and leaves of trees. They are entirely ignorant of agriculture, and keep no poultry or domestic animals. They make rude pots of clay, sun-dried, or partially baked in the fire, but these are hand-made, as they are ignorant of the use of the potter's wheel. Their clothing is of the scantiest description, and what little they have serves chiefly for decorative or ornamental purposes, and not for keeping the body warm. They make no use of the skins of animals. They have fairly well-made dug-out canoes and outriggers, but fit only for navigating the numerous creeks and straits between the islands, and not for voyages in the open sea. They are expert swimmers and divers. Though constantly using fire, they are quite ignorant of the art of producing it, and have to expend much care and labour in keeping up a constant supply of burning or smouldering wood. They are ignorant of all metals; but for domestic purposes make great use of shells, especially a species of *Cyrene* found abundantly on the shores of the islands, also quartz chips and flakes, and bamboo knives. They have stone anvils and hammers, and they make good string from vegetable fibres, as well as baskets, fishing nets, sleeping mats, &c. Their principal weapons are the bow and arrow, in the use of which they are very skilful. They have harpoons for killing turtle and fish, but no kind of shield or breastplate for defence when fighting. The natural

* See "On the Osteology and Affinities of the Natives of the Andaman Islands" ('Journal Anthropological Institute,' vol. ix. p. 108, 1879); and "Additional Observations on the Osteology of the Natives of the Andaman Islands" (ibid. vol. xiv. p. 115, 1884).

fertility of the island supplies them with abundance and variety of food all the year round, the purveying of which affords occupation and amusement for the greater part of the male population. This consists of pigs (*Sus andamanensis*), which are numerous on the islands, paradoxures, dugong. and occasionally porpoise, iguanas, turtles, turtles' eggs, many kinds of fish, prawns, mollusks, larvæ of large wood-boring and burrowing beetles, honey, and numerous roots (as yams), fruits, and seeds. The food is invariably cooked before eating, and generally taken when extremely hot. They were ignorant of all stimulants or intoxicating drinks—in fact, water was their only beverage; and tobacco, or any substitute for it, was quite unknown till introduced by Europeans.

As with all other human beings existing at present in the world, however low in the scale of civilisation, the social life of the Andamanese is enveloped in a complex maze of unwritten law or custom, the intricacies of which are most difficult for any stranger to unravel. The relations they may or may not marry, the food they are obliged or forbidden to partake of at particular epochs of life or seasons of the year, the words and names they may or may not pronounce; all these, as well as their traditions, superstitions, and beliefs, their occupations, games, and amusements, of which they seem to have had no lack, would take far too long to describe here; but before leaving these interesting people, I may quote an observation of Mr. Man's, which, unless he has seen them with too *couleur-de-rose* eyesight, throws a very favourable light upon the primitive unsophisticated life of these poor little savages, now so ruthlessly broken into and destroyed by the exigencies of our ever-extending empire.

"It has been asserted," Mr. Man says, "that the 'communal marriage' system prevails among them, and that 'marriage is nothing more than taking a female slave'; but, so far from the contract being regarded as a merely temporary arrangement, to be set aside at the will of either party, no incompatibility of temper or other cause is allowed to dissolve the union; and while bigamy, polygamy, polyandry, and divorce are unknown, conjugal fidelity till death is not the exception but the rule, and matrimonial differences, which, however, occur but rarely, are easily settled with or without the intervention of friends." In fact, Mr. Man goes on to say, "One of the most striking features of their social relations is the marked equality and affection which subsists between husband and wife," and "the consideration and respect with which women are treated might with advantage be emulated by certain classes in our own land."

It should also be mentioned that cannibalism and infanticide, two such common incidents of savage life, were never practised by them.

We must now pass to the important scientific question, Who are the natives of the Andaman Islands, and where, among the other races of the human species, shall we look for their nearest relations?

It is due mainly to the assiduous researches into all the documentary evidence relating to the inhabitants of Southern Asia and

the Indian Archipelago, conducted through many years by M. de Quatrefages, in some cases with the assistance of his colleague M. Hamy, that the facts I am about to put before you have been prominently brought to light, and their significance demonstrated.

It is well known that the greater part of the large island of New Guinea, and of the chain of islands extending eastwards and southwards from it, including the Solomon Islands, the New Hebrides, and New Caledonia, and also the Fijis, are still inhabited mainly by people of dark colour, frizzly hair, and many characteristics allying them to the negroes of Africa. These constitute the race to which the term Melanesian is commonly applied in this country, or Oceanic negroes, the "Papouas" of Quatrefages. Their area at one time was more extensive than it is now, and has been greatly encroached upon by the brown, straight-haired Polynesian race with Malay affinities, now inhabiting many of the more important islands of the Pacific, and the mingling of which with the more aboriginal Melanesians in various proportions has been a cause, among others, of the diverse aspect of the population on many of the islands in this extensive region. These Papouas, or Melanesians, however, differ greatly from the Andamanese in many easily defined characters, which are, especially, their larger stature, their long, narrow, and high skulls, and their coarser and more negro-like features. Although undoubtedly allied, we cannot look to them as the nearest relations of our little Andamanese.

When the Spaniards commenced the colonisation of the Philippines, they met with, in the mountainous region in the interior of the Island of Luzon, besides the prevailing native population, consisting of Tagals of Malay origin, very small people, of black complexion, with the frizzly hair of the African negroes. So struck were they with the resemblance, that they called them "Negritos del Monte" (little Negroes of the mountain). Their local name was Aigtas, or Inagtas, said to signify "black," and from which the word Aëta, generally now applied to them, is derived. These people have lately been studied by two French travellers, M. Marche and Dr. Montano; the result of their measurements gives 4 feet 8¼ inches as the average height of the men, and 4 feet 6½ inches the average for the women. In many of their moral characteristics they resemble the Andamanese. The Aëtas are faithful to their marriage vows, and have but one wife. The affection of parents for children is very strong, and the latter have for their father and mother much love and respect. The marriage ceremony, according to M. Montano, is very remarkable. The affianced pair climb two flexible trees placed near to each other. One of the elders of the tribe bends them towards each other. When their heads touch, the marriage is legally accomplished. A great *fête*, with much dancing, concludes the ceremony.

It was afterwards found that the same race existed in other parts of the archipelago, Panay, Mindanao, &c., and that they entirely

peopled some little islands—among others, Bougas Island, or "Isla de los Negros."

As the islands of these eastern seas have become better known, further discoveries of the existence of a small Negroid population have been made in Formosa, in the interior of Borneo, Sandalwood Island (Sumba), Xulla, Bourou, Ceram, Flores, Solor, Lomblem, Pantar, Ombay, the eastern peninsula of Celebes, &c. In fact, Sumatra and Java are the only large islands of this great area which contain no traces of them except some doubtful cross-breeds, and some remains of an industry which appears not to have passed beyond the Age of Stone.

The Sunda Islands form the southern limit of the Negrito area; Formosa, the last to the north, where the race has preserved all its characters. But beyond this, as in Loo Choo, and even in the south-east portion of Japan, it reveals its former existence by the traces it has left in the present population. That it has contributed considerably to form the population of New Guinea is unquestionable. In many parts of that great island, small round-headed tribes live more or less distinct from the larger and longer-headed people who make up the bulk of the population.

But it is not only in the islands that the Negrito race dwell. Traces of them are found also on the mainland of Asia, but everywhere under the same conditions: in scattered tribes, occupying the more inaccessible mountainous regions of countries otherwise mainly inhabited by other races, and generally in a condition more or less of degradation and barbarism, resulting from the oppression with which they have been treated by their invading conquerers; often moreover, so much mixed that their original characters are scarcely recognisable. The Semangs of the interior of Malacca in the Malay peninsula, the Sakays from Perak, the Moys of Annam, all show traces of Negrito blood. In India proper, especially among the lowest and least civilised tribes, not only of the central and southern districts, but almost to the foot of the Himalayas, in the Punjab, and even to the west side of the Indus, according to Quatrefages, frizzly hair, negro features, and small stature, are so common that a strong argument can be based on them for the belief in a Negrito race forming the basis of the whole pre-Aryan, or Dravidian as it is generally called, population of the peninsula. The crossing that has taken place with other races has doubtless greatly altered the physical characters of this people, and the evidences of this alteration manifest themselves in many ways; sometimes the curliness of the hair is lost by the admixture with straight-haired races, while the black complexion and small stature remain; sometimes the stature is increased, but the colour, which seems to be one of the most persistent of characteristics, remains.

The localities in which the Negrito people are found in their greatest purity, either in almost inaccessible islands, as on the Andamans, or elsewhere in the mountainous ranges of the interior only; and their social condition and traditions, wherever they exist—all point to the fact that they were the earliest inhabitants; and that the Mongolian

and Malay races on the east, and the Aryans on the west, which are now so rapidly exterminating and replacing them, are later comers into the land, exactly as, in the greater part of the Pacific Ocean, territory formerly occupied by the aboriginal dark, frizzly-haired Negroid Melanesians has been gradually and slowly invaded by the brown Polynesians, who in their turn, but by a much more rapid process, are being replaced by Europeans.

We now see what constitutes the great interest of the Andamanese natives to the student of the ethnological history of the Eastern world. Their long isolation has made them a remarkably homogeneous race, stamping them all with a common resemblance not seen in the mixed races generally met with in continental areas. For although, as with most savages, marriages within the family (using the term in a very wide sense) are most strictly forbidden, all such alliances have necessarily been confined to natives of the islands. They are the least modified representatives of the people who were, so far as we know, the primitive inhabitants of a large portion of the earth's surface, but who are now verging on extinction. It is, however, not necessary to suppose that the Andaman Islanders give us the exact characters and features of all the other branches of the race. Differences in detail doubtless existed—differences which are almost always sure to arise whenever races become isolated from each other for long periods of time.

In many cases the characters of the ancient inhabitants of a land have been revealed to us by the preservation of their actual remains. Unfortunately we have as yet no such evidence to tell us of the former condition of man in Southern Asia. We may, however, look upon the Andamanese, the Aëtas, and the Semangs, as living fossils; and by their aid conjecture the condition of the whole population of the land in ancient times. It is possible, also, to follow Quatrefages, and to see in them the origin of the stories of the Oriental pygmies related by Ctesias and by Pliny.

We now pass to the continent of Africa, in the interior of which the pygmies of Homer, Herodotus, and Aristotle have generally been placed. Africa, as is well known, is the home of another great branch of the black, frizzly-haired, or Ethiopian division of the human species, which does, or did till lately, occupy the southern two-thirds of this great continent, the northern third being inhabited by Hamite and Semite branches of the great white or Caucasian primary division of the human species, or by races resulting from the mixture of these with the Negroes. But besides the true Negro, there has long been known to exist in the southern part of the continent a curiously modified type, consisting of the Hottentots, and the Bushmen—Bosjesmen (men of the woods) of the Dutch colonists—the latter of whom, on account of their small size, come within the scope of the present subject. They lead the lives of the most degraded of savages, dwelling among the rocky and more inaccessible mountains of the interior, making habitations of the natural caves, subsisting entirely by the

chase, being most expert in the use of the bow and arrow, and treated as enemies and outcasts by the surrounding and more civilised tribes, whose flocks and herds they show little respect for when other game is not within reach. The physical characters of these people are well known, as many specimens have been brought to Europe alive for the purpose of exhibition. The hair shows the extreme of the frizzly type; being shorter and less abundant than that of the ordinary Negro, it has the appearance of growing in separate tufts, which coil up together into round balls compared to "peppercorns." The yellow complexion differs from that of the Negro, and, combined with the wide cheek-bones and form of the eyes, so much recalls that of certain of the pure yellow races that some anthropologists are inclined to trace true Mongolian affinities and admixture, although the extreme crispness of the hair makes such a supposition almost impossible. The width of the cheek-bones and the narrowness of the forehead and the chin give a lozenge-shape to the front view of the face. The forehead is prominent and straight; the nose extremely flat and broad, more so than in any other race, and the lips prominent and thick, although the jaws are less prognathous than in the true Negro races. The cranium has many special characters by which it can be easily distinguished from that of any other. It has generally a very feminine, almost infantile, appearance, though the capacity of the cranial cavity is not the smallest, exceeding that of the Andamanese. In general form the cranium is rather oblong than oval, having straight sides, a flat top, and especially a vertical forehead, which rises straight from the root of the nose. It is moderately dolichocephalic or rather mesaticephalic, the average index of ten specimens being 75·4. The height is in all considerably less than the breadth, the average index being 71·1. The glabella and infra-orbital ridges are little developed, except in the oldest males. The malar bones project much forwards, and the space between the orbits is very wide and flat. The nasal bones are extremely small and depressed, and the aperture wide; the average nasal index being 60·8, so they are the most platyrhine of races.

With regard to the stature, we have not yet sufficient materials for giving a reliable average. Quatrefages, following Barrow, gives 4 feet 6 inches for the men, and 4 feet for the women, and speaks of one individual of the latter sex, who was the mother of several children, measuring only 3 feet 9 inches in height, but later observations (still, however, insufficient in number) give a rather larger stature: thus Topinard places the average at 1·404 metre, or 4 feet 7½ inches; and Fritsch, who measured six male Bushmen in South Africa, found their mean height to be 1·444 metre, or nearly 4 feet 9 inches. It is probable that, taking them all together, they differ but little in size from the Andamanese, although in colour, in form of head, in features, and in the proportions of the body, they are widely removed from them.

There is every reason to believe that these Bushmen represent the

earliest race of which we have, or are ever likely to have, any knowledge, which inhabited the southern portion of the African continent, but that long before the advent of Europeans upon the scene, they had been invaded from the north by Negro tribes, who, being superior in size, strength, and civilisation, had taken possession of the greater part of their territories, and mingling freely with the aborigines, had produced the mixed race called Hottentots, who retained the culture and settled pastoral habits of the Negroes, with many of the physical features of the Bushmen. These, in their turn, encroached upon by the pure-bred Bantu Negroes from the north, and by the Dutch and English from the south, are now greatly diminished, and indeed threatened with the same fate that will surely soon befall the scanty remnant of the early inhabitants who still retain their primitive type.

At present the habitat of the Bushman race is confined to certain districts in the south-west of Africa, from the confines of the Cape Colony as far north as the shores of Lake Ngami. Further to the north the great equatorial region of Africa is occupied by various Negro tribes, using the term in its broadest sense, but belonging to the divisions which, on account of peculiarities of language, have been grouped together as Bantu. They all present the common physical characteristics typical of the Negro race, only two of which need be specially mentioned here — medium or large stature, and dolichocephalic skull (average cranial index about 73·5).

It is at various scattered places in the midst of these, that the only other small people of which I shall have to speak, the veritable pygmies of Homer, Herodotus, and Aristotle, according to Quatrefages, are still to be met with.*

The first notice of the occurrence of these in modern times is contained in "The strange adventures of Andrew Battell of Leigh in Essex, sent by the Portugals prisoner to Angola, who lived there, and in the adjoining regions near eighteen years" (1589 to 1607), published in 'Purchas his Pilgrimes' (1625), lib. vii. chap. iii. p. 983:—

"To the north-east of *Mani-Kesock*, are a kind of little people, called *Matimbas;* which are no bigger than Boyes of twelve yeares old, but very thicke, and live only upon flesh, which they kill in the woods with their bows and darts. They pay tribute to *Mani-Kesock*, and bring all their Elephants' teeth and tayles to him. They will not enter into any of the *Maramba's* houses, nor will suffer any to come where they dwell. And if by chance any *Maramba* or people of *Longo* pass where they dwell, they will forsake that place and go to another. The women carry Bows and Arrows as well as the men. And one of these will walk in the woods alone and kill the Pongos with their poysoned Arrows."

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* The scattered information upon this subject was first collected together by Hamy in his "Essai de co-ordination des Matériaux récemment recueillis sur l'ethnologie des Négrilles ou Pygmées de l'Afrique équatoriale," 'Bull. Soc. d'Anthropologie de Paris,' tome ii. (ser. iii.), 1879, p. 79.

Battell's narrative, it should be said, is generally admitted as having an air of veracity about it not always conspicuous in the stories of travellers of his time. In addition to the observations on the human inhabitants, it contains excellent descriptions of animals, as the pongo or gorilla, and the zebra, now well known, but in his day new to Europeans.

Dapper, in a work called 'Description de la Basse Ethiopie,' published in Amsterdam in 1686, speaks of a race of dwarfs inhabiting the same region, which he calls *Mimos* or *Bakke-Bakke*, but nothing further was heard of these people until quite recent times. A German scientific expedition to Loango, the results of which were published in the 'Zeitschrift für Ethnologie,' 1874, and in Hartmann's work, 'Die Negritier," obtained, at Chinchoxo, photographs and descriptions of a dwarf tribe called "Baboukos," whose heads were proportionally large and of roundish form (cephalix index of skull, 78 to 81). One individual, supposed to be about forty years of age, measured 1·365 metres, rather under 4 feet 6 inches.

Dr. Touchard, in a "Notice sur le Gabon," published in the 'Revue Maritime et Coloniale' for 1861, describes the recent destruction of a population established in the interior of this country, and to which he gives the name of "Akoa." They seem to have been exterminated by the M'Pongos in their expansion towards the west. Some of them, however, remained as slaves at the time of the visit of Admiral Fleuriot de Langle, who in 1868 photographed one (measuring about 4 feet 6 inches high) and brought home some skulls, which were examined by Hamy, and all proved very small and sub-brachycephalic.

Another tribe, the M'Boulous, inhabiting the coast north of the Gaboon river, have been described by M. Marche as probably the primitive race of the country. They live in little villages, keeping entirely to themselves, though surrounded by the larger negro tribes, M'Pongos and Bakalais, who are encroaching upon them so closely that their numbers are rapidly diminishing. In 1860 they were not more than 3000; in 1879 they were much less numerous. They are of an earthy-brown colour, and rarely exceed 1·600 metre in height (5 feet 3 inches). In the rich collections of skulls made by Mr. R. B. Walker and by M. Du Chaillu, from the coast of this region, are many which are remarkable for their small size and round form. Of many other notices of tribes of negroes of diminutive size, living near the west coast of Equatorial Africa, I need only mention that of Du Chaillu, who gives an interesting account of his visit to an Obongo village in Ashango-land, between the Gaboon and the Congo; although unfortunately, owing to the extreme shyness and suspicion of the inhabitants, he was allowed little opportunity for anthropological observations. He succeeded, however, in measuring one man and six women; the height of the former was 4 feet 6 inches, the average of the latter 4 feet 8 inches.*

* 'A Journey to Ashango-land,' 1867, p. 315.

Far further into the interior, towards the centre of the region contained in the great bend of the Congo or Livingstone River, Stanley heard of a numerous and independent population of dwarfs, called "Watwas," who, like the Batimbas of Battell, are great hunters of elephants, and use poisoned arrows. One of these he met with at Ikondu, was 4 feet 6½ inches high, and of a chocolate brown colour.* More recently Dr. Wolff describes under the name of "Batouas" (perhaps the same as Stanley's Watwas), a people of lighter colour than other negroes, and never exceeding 1·40 metres (4 feet 7 inches) high, but whose average is not more than 1·30 (4 feet 3 inches), who occupy isolated villages scattered through the territory of the Bahoubas, with whom they never mix.†

Penetrating into the heart of Africa from the north-east, in 1870, Dr. Schweinfurth first made us acquainted with a diminutive race of people who have since attained a considerable anthropological notoriety. They seem to go by two names in their own country, *Akka* and *Tikki-tikki*, the latter reminding us curiously of Dapper's Bakke-bakke, and the former, more singularly still, having been read by the learned Egyptologist, Mariette, by the side of the figure of a dwarf in one of the monuments of the early Egyptian empire.

It was at the court of Mounza, king of the Monbuttu, that Schweinfurth first met with the Akkas. They appear to live under the protection of that monarch, who had a regiment of them attached to his service, but their real country was further to the south and west, about 3° N. lat. and 25° E. long. From the accounts the traveller received, they occupy a considerable territory, and are divided into nine distinct tribes, each having its own king or chief. Like all the other pygmy African tribes, they live chiefly by the chase, being great hunters of the elephant, which they attack with bows and arrows.

In exchange for one of his dogs, Schweinfurth obtained from Mounza one of these little men, whom he intended to bring to Europe, but who died on the homeward journey at Berber. Unfortunately all the measurements and observations which were made in the Monbuttu country by Schweinfurth perished in the fire which destroyed so much of the valuable material he had collected. His descriptions of their physical characters are therefore chiefly recollections. Other travellers—Long, Marno, and Vossion—though not penetrating as far as the Akka country, have given observations upon individuals of the race they have met with in their travels. The Italian Miani, following the footsteps of Schweinfurth into the Monbuttu country, also obtained by barter two Akka boys, with the view of bringing them to Europe. He himself fell a victim to the fatigues of the journey and climate, but left his collections, including the young Akkas, to the Italian Geographical Society. Probably no

* 'Through the Dark Continent,' vol. ii.

† 'La Gazette Géographique,' 1887, p. 153, quoted by Quatrefages.

two individuals of a savage race have been so much honoured by the attentions of the scientific world. First at Cairo, and afterwards in Italy, Tebo (or Thibaut) and Chairallah, as they were named, were described, measured, and photographed, and have been the subjects of a library of memoirs, their bibliographers including the names of Owen, Panceri, Cornalia, Mantegazza, Giglioli and Zannetti, Broca, Hamy, and de Quatrefages. On their arrival in Italy, they were presented to the king and queen, introduced into the most fashionable society, and finally settled down as members of the household of Count Miniscalchi Erizzo, at Verona, where they received a European education, and performed the duties of pages.

In reply to an inquiry addressed to my friend Dr. Giglioli, of Florence, I hear that Thibaut died of consumption on January 28th, 1883, being then about twenty-two years of age, and was buried in the cemetery at Verona. Unfortunately no scientific examination of the body was allowed, but whether Chairallah still lives or not I have not been able to learn. As Giglioli has not heard of his death, he presumes that he is still living in Count Miniscalchi's palace.

One other specimen of this race has been the subject of careful observation by European anthropologists—a girl named Saida, brought home by Romolo Gessi (Gordon's lieutenant), and who is still, or was lately, living at Trieste as servant to M. de Gessi.

The various scattered observations hitherto made are obviously insufficient to deduce a mean height for the race, but the nearest estimate that Quatrefages could obtain is about 4 feet 7 inches for the men, and 4 feet 3 inches for the women, decidedly inferior, therefore, to the Andamanese. With regard to their other characters, their hair is of the most frizzly kind, their complexion lighter than that of most Negroes, but the prognathism, width of nose, and eversion of lips characteristic of the Ethiopian branch of the human family are carried to an extreme degree, especially if Schweinfurth's sketches can be trusted. The only essential point of difference from the ordinary Negro, except the size, is the tendency to shortening and breadth of the skull, although it by no means assumes the "almost spherical" shape attributed to it by Schweinfurth.

Some further information about the Akkas will be found in the work, just published, of the intrepid and accomplished traveller, in whose welfare we are now so much interested, Dr. Emin Pasha, Gordon's last surviving officer in the Soudan, who, in the course of his explorations, spent some little time lately in the country of the Monbuttu. Here he not only met with living Akkas, one of whom he apparently still retains as a domestic in his service, and of whose dimensions he has sent me a most detailed account, but he also, by watching the spots where two of them had been interred, succeeded in obtaining their skeletons, which, with numerous other objects of great scientific interest, safely arrived at the British Museum in September of last year. I need hardly say that actual bones, clean, imperishable, easy to be measured and compared, not once only, but

any number of times, furnish the most acceptable evidence that an anthropologist can possess of many of the most important physical characters of a race. There we have facts which can always be appealed to in support of statements and inferences based on them. Height, proportions of limbs, form of head, characters of the face even, are all more rigorously determined from the bones than they can be on the living person. Therefore, the value of these remains, imperfect as they unfortunately are, and of course insufficient in number for the purpose of establishing average characters, is very great indeed.

As I have entered fully into the question of their peculiarities elsewhere,* I can only give now a few of the most important and most generally to be understood results of their examination. The first point of interest is their size. The two skeletons are both those of full-grown people, one a man, the other a woman. There is no reason to suppose that they were specially selected as exceptionally small; they were clearly the only ones which Emin had an opportunity of procuring; yet they fully bear out, more than bear out, all that has been said of the diminutive size of the race. Comparing the dimensions of the bones, one by one, with those of the numerous Andamanese that have passed through my hands, I find both of these Akkas smaller, not than the average, but smaller than the smallest; smaller also than any Bushman whose skeleton I am acquainted with, or whose dimensions have been published with scientific accuracy. In fact, they are both, for they are nearly of a size, the smallest normal human skeletons which I have seen, or of which I can find any record. I say normal, because they are thoroughly well-grown and proportioned, without a trace of the deformity almost always associated with individual dwarfishness in a taller race. One only, that of the female, is sufficiently perfect for articulation. After due allowance for some missing vertebræ, and for the intervertebral spaces, the skeleton measures from the crown of the head to the ground exactly 4 feet, or 1·218 metre. About half an inch more for the thickness of the skin of the head and soles of the feet would complete the height when alive. The other (male) skeleton was (judging by the length of the femur) about a quarter of an inch shorter.

The full-grown woman of whom Emin gives detailed dimensions is stated to be only 1·164 metre, or barely 3 feet 10 inches.† These heights are all unquestionably less than anything that has been yet obtained based upon such indisputable data. One very interesting

* In a paper read before the Anthropological Institute of Great Britain and Ireland, February 14th, 1888, which will be published in the August number of the Journal.

† In his letters Emin speaks of an Akka man as "3 feet 6 inches" high though this does not profess to be a scientific accurate observation, as does the above. He says of this man that his whole body was covered by thick, stiff hair, almost like felt, as was the case with all the Akkas he had yet examined.

and almost unexpected result of a careful examination of these skeletons is that they conform in the relative proportions of the head, trunk, and limb, not to dwarfs, but to full-sized people of other races, and they are therefore strikingly unlike the stumpy, long-bodied, short-limbed, large-headed pygmies so graphically represented fighting with their lances against the cranes on ancient Greek vases.

The other characters of these skeletons are Negroid to an intense degree, and quite accord with what has been stated of their external appearance. The form of the skull, too, has that sub-brachycephaly which has been shown by Hamy to characterise all the small Negro populations of Central Africa. It is quite unlike that of the Andamanese, quite unlike that of the Bushmen. They are obviously Negroes of a special type, to which Hamy has given the appropriate term of *Negrillo.* They seem to have much the same relation to the larger long-headed African Negroes that the small round-headed Negritos of the Indian Ocean have to their larger long-headed Melanesian neighbours.

At all events, the fact now seems clearly demonstrated that at various spots across the great African continent, within a few degrees north and south of the equator, extending from the Atlantic coast to near the shores of the Albert Nyanza (30° E. long.), and perhaps, from some indications which time will not allow me to enter into now (but which will be found in the writings of Hamy and Quatrefages), even further to the east, south of the Galla land, are still surviving, in scattered districts, communities of these small Negroes, all much resembling each other in size, appearance, and habits, and dwelling mostly apart from their larger neighbours, by whom they are everywhere surrounded. Our information about them is still very scanty, and to obtain more would be a worthy object of ambition for the anthropological traveller. In many parts, especially at the west, they are obviously holding their own with difficulty, if not actually disappearing, and there is much about their condition of civilisation, and the situations in which they are found, to induce us to look upon them, as in the case of the Bushmen in the south and the Negritos in the east, as the remains of a population which occupied the land before the incoming of the present dominant races. If the account of the Nasamonians related by Herodotus be accepted as historical, the river they came to, " flowing from west to east," must have been the Niger, and the northward range of the dwarfish people far more extensive twenty-three centuries ago than it is at the present time.

This view opens a still larger question, and takes us back to the neighbourhood of the south of India as the centre from which the whole of the great Negro race spread, east over the African continent, and west over the islands of the Pacific, and to our little Andamanese fellow subjects as probably the least modified descendants of the primitive members of the great branch of the human species characterised by their black skins and frizzly hair.

[W. H. F.]

WEEKLY EVENING MEETING,

Friday, April 20, 1888.

Edward Woods, Esq. M. Inst. C.E. Vice-President, in the Chair.

The Right. Hon. Sir William R. Grove, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*

Antagonism.

Some months ago, shortly after I had resigned my office of Judge of the High Court, I was expressing to a friend my fear of the effect of having no compulsory occupation, when he said, by way of consolation, "Never mind, 'for Satan finds some mischief still for idle hands to do.'" You may possibly in the course of this evening think he was right.

I have chosen a title for my lecture which may not fully convey to your minds the scope of the views which I am going to submit to you. I propose to adduce some arguments to show that "antagonism," a word generally used to signify something disagreeable, pervades all things; that it is not the baneful thing which many consider it; that it produces at least quite as much good as evil; but that, whatever be its effect, my theory—call it, if you will, speculation—is that it is a necessity of existence, and of the organism of the universe so far as we understand it; that motion and life cannot go on without it; that it is not a mere casual adjunct of Nature, but that without it there would be no Nature, at all events as we conceive it; that it is inevitably associated with unorganised matter, with organised matter, and with sentient beings.

I am not aware that this view, in the breadth in which I suggest it, has been advanced before. Probably no idea is new in all respects in the present period of the world's history. It has been said by a desponding pessimist that "There is nothing new, and nothing true, and nothing signifies," but I do not entirely agree with him; I believe that in what I am about to submit there is something new and true in the point of view from which I regard the matter; whether it signifies or not is for you to judge.

The universality of antagonism has not received the attention it seems to me to deserve from the fact of the element of force, or rather of the conquering force, being mainly attended to, and too little note taken of the element of resistance unless the latter vanquishes the force, and then it becomes, popularly speaking, the force, and the former force the resistance.

There are propositions applying more or less to what I am going to say of some antiquity.

Heraclitus, quoted by Prof. Huxley, said: "War is the father and king of all things." Hobbes said war is the natural state of man, but his expressions have about them some little ambiguity. In Chapter I. of the 'De Corpore Politico' he says, "Irresistible might in a state of nature is right," and "The estate of man in this natural liberty is war." Subsequently he says: "A man gives up his natural right, for when divers men having right not only to all things else, but to one another's persons, if they use the same there ariseth thereby invasion on the one part and resistance on the other, which is war, and therefore *contrary to the law of Nature, the sum whereof consisteth in making peace.*" I can only explain this apparent inconsistency by supposing he meant "law of Nature" to be something different from "the natural estate of man," and that the making peace was the first effort at contract, or the beginning of law; but then why call it the "*law of Nature,*" where he says might is right? There is some obscurity in the passage.

The Persian divinities, Ormuzd and Ahriman, were the supposed rulers or representatives of good and evil, always at war, and causing the continuous struggles between human beings animated respectively by these two principles. Undoubtedly good and evil are antagonistic, but antagonism, as I view it, is as necessary to good as to evil, as necessary to Ormuzd as to Ahriman. Zoroaster's religion of a Divine being, one and indivisible, but with two sides, is, to my mind, a more philosophical conception. The views of Lamarck on the modification of organic beings by effort, and the establishment of the doctrine of Darwin as to the effects produced by the struggle for existence and domination, come much nearer to my subject. Darwin has shown how these struggles have modified the forms and habits of organised beings, and tended to increased differentiation, and Prof. Huxley and Herbert Spencer have powerfully promoted and expanded these doctrines. To the latter we owe the happy phrase, "survival of the fittest," and Prof. Huxley has recently, in a paper in the 'Nineteenth Century,' anticipated some points I should have adverted to as to the social struggles for existence. To be anticipated, and by a very short period, is always trying, but it is more trying when what you intended to say has been said by your predecessor in more terse and appropriate language than you have at your command.

I propose to deal with "antagonism" inductively, i.e. with facts derived from observation alone, and not to meddle with spiritual matters or with consequences.

Let us begin with what we know of the visible universe, viz. suns, planets, comets, meteorites, and their effects. These are all pulling at each other, and resisting that pull by the action of other forces.

Any change in this pulling force produces a change, or, as it is called, perturbation, in the motion of the body pulled. The planet

Neptune, as you know, was discovered by the effect of its pulling force on another planet, the latter being deflected from its normal course. When this pulling force is not counterbalanced by other forces, or when the objects pulled have not sufficient resisting power, they fall into each other. Thus, this earth is daily causing a bombardment of itself by drawing smaller bodies—meteorites—to it, 20,000,000 of which, visible to the naked eye, fall on an average into our atmosphere in each twenty-four hours, and of those visible through the telescope, 400,000,000 are computed to fall within the same period. Mr. Lockyer has recently given reasons for supposing the luminosity of nebulæ, or of many of them, is due to collisions or friction among the meteorites which go to form them; but his paper on the subject is not yet published. You must get from Mr. Lockyer the details of his views. I hope he may, at one of these evening meetings, give you a *résumé* of them from the place I now occupy.

What is commonly called centrifugal force does not come from nothing; it depends upon the law that a body falling by the influence of attraction, not upon, but near to, the attracting body, whirls round the latter, describing one of the curves known as conic sections. Hence a meteorite may become a planet or satellite (one was supposed to have become so to this earth, but I believe the observations have not been verified); or it may go off in a parabola as comets do; or again, this centrifugal force may be generated by the gradual accretion of nebulous matter into solid masses falling near to, or being thrown off from the central nucleus, the two forces (centrifugal and centripetal) being antagonistic to each other, and the relative movements being continuous, but probably not perpetual. Our solar system is also kept in its place by the antagonism of the surrounding bodies of the Kosmos pulling at us. Suppose half of the stars we see, i. e. all on one side of a meridian line, were removed, what would become of our solar system? It would drift away to the side where attraction still existed, and there would be a wreck of matter and a crash of worlds. It is very little known that Shakespeare was acquainted with this pulling force. He says, by the mouth of Cressida—

> "But the strong base and building of my love
> Is as the very centre of the earth,
> Drawing all things to it"—

a very accurate description of the law of gravitation, so far as this earth is concerned, and written nearly a century before Newton's time.

But in all probability the collisions of meteorites with the earth and other suns and planets are not the only collisions in space. I know of no better theory to account for the phenomena of temporary stars, such as that which appeared in 1866, than that they result from the collision of non-luminous stars, or stars previously invisible to us. That star burst suddenly into light, and then the luminosity gradually faded, the star became more and more dim, and ultimately disappeared. The spectrum of it showed that the light was compound, and had pro-

bably emanated from two different sources. It was probably of a very high temperature. If this theory of temporary stars be admitted, we get a nebula of vapour or star dust again, and so may get fresh instances of the nebular hypothesis.

Let us now take the earth itself. It varies in temperature, and consequently the particles at or near its surface are in continuous movement, rubbing against each other, being oxidized or deoxidized, either immediately or through the medium of vegetation. This also is continuously tearing up its surface and changing its character. Evaporation and condensation, producing rain, hail, and storms, notably change it. Force and resistance are constantly at play. The sea erodes rocks and rubs them into sand. The sea quits them, and leaves traces of its former presence by the fossil marine shells found now at high altitudes. Rocks crumble down and break other rocks or are broken by them; avalanches are not uncommon. The interior of the earth seems to be in a perpetual state of commotion, though only recurrent to our observation. Earthquakes in various places from time to time, and doubtless many beneath the sea of which we are not cognizant, nor of other gradual upheavals and depressions. Throughout it nothing that we know of is at rest, and nothing can move without changing the position of something else, and this is antagonism. Metals rust at its surface, and probably they or their oxides, chlorides, &c., are in a continuous state of change in the interior. Nothing that we know of is stationary. The earth as a whole seems so at first sight, but its surface is moving at the rate of some seventeen miles a minute at the equator; and standing at either of the poles—an experiment which no one has yet had an opportunity of trying—a man would be turned round his own axis once in every twenty-four hours, while the earth's motion round the sun carries us through space more than a million and a half of miles a day.

The above changes produce motion in other things. The earth pulls the sun and planets, and in different degrees at different portions of its orbit.

Before I pass from inorganic to organized matter I had better deal with what may perhaps strike you as the most difficult part of my subject, viz. light. Where, you may say, is there antagonism in the case of light? Light exercises its force upon such minute portions of matter that until the period of the discovery of photography its physical and chemical effects were almost unknown. Such effects as bleaching, uniting some gases, and affecting the colouring matter of vegetables, were partially known but little attended to; but photography created a new era: I shall advert to this presently. The theories of light, however, involved matter and motion. The corpuscular theory, as you well know, supposed that excessively small particles were emitted from luminous bodies, and travelled with enormous velocity. The undulatory theory, which supplanted it, supposed that luminous bodies caused undulations or vibrations in a highly tenuous matter called ether, which is supposed to exist

throughout the interplanetary spaces and throughout the universe so far as we know it. Some suppose this ether to be of a specific character differing from that of ordinary gases, others that it is in the nature of a highly attenuated gas; but, whatever it be, it cannot be affected by undulations or vibrations without being moved, and when matter is moved by any force it must offer resistance to that force, and hence we get antagonism between force and resistance. Light also takes time in overcoming this resistance, i. e. in pushing aside the ether. It travels, no doubt, at a good pace—about 190,000 miles in a second; but even at this rate, and without being particular as to a few millions of miles, it takes three years and a quarter to reach us from the star which, so far as we know, is the nearest to us, viz. α Centauri. The ether, or whatever it may be called, tenuous as it is, is not unimportant, though it be not heavy. Without it we should have no light and possibly no heat, and the consequences of its absence would be rather formidable. I believe you have heard Dr. Tyndall on this subject. Supposing the visible universe to be as it is now supposed to be, i. e. in no part a mere vacuum, there can be no force without resistance in any part of it.

But photography carries us further, it shows us that light acts on matter chemically, that it is capable of decomposing or forcing asunder the constituents of chemical compounds, and is therefore a force met by resistance. In the year 1856 I made some experiments, published in the 'Philosophical Magazine' for January 1857, which seemed to me to carry still further what I may call the molecular fight between light and chemical affinity, and among them the following. Letters cut out of paper are placed between two polished squares of glass with tin-foil on the outsides. It is then electrized like a Leyden jar, for a few seconds, the glasses separated, the letters blown off, and the inside of one of the glasses covered with photographic collodion. This is then exposed to diffuse daylight, and on being immersed in the nitrate of silver bath the part which had been covered with the paper comes out dark, the remainder of the plate being unaffected. (This result was shown by the electric light lantern.) In this case we see that another imponderable force, electricity, invisibly affects the surface of glass in such a way that it conveys to another substance of definite thickness, viz. the prepared collodion, a change in the chemical relations of the substance (iodide of silver) pervading it, enabling it to resist that decomposition by light which but for some unseen modification of the surface of the glass plate it would have undergone, and no doubt the force of light being unable to effect its object was reflected or dispersed, and instead of changing its mode of motion in effecting chemical decomposition, it goes off on other business. The visible effect is in the collodion film alone. I have stripped that off, and the imprint remains on it, the surface of the glass being, so far as I could ascertain, unaffected. Thus in the film over the protected part, light conquers chemical affinity; in that over the non-protected part, chemical affinity resists and conquers light,

which has to make an ignominious retreat. It is a curious chapter in the history of the struggles of molecular forces, and probably similar contests between light and chemical or physical attractions go on in many natural phenomena, some forms of blight and some healthy vegetable changes being probably dependent on the varying effects of light, and conditions, electrical or otherwise, of the atmosphere.

Let us now pass on to organic life. A blade of grass, as Burke, I believe, said as a figure of speech, is fighting with its neighbours. It is robbing them, and they are trying to rob it—no agreement or contract, simply force opposed to force. This struggle is good for the grass; if it got too much nutriment it would become diseased. The struggle keeps it in health. The rising of sap in trees, the assimilation of carbon, the process of growth, the strengthening themselves to resist prevalent winds, and many other instances might be given, which afford examples of the internal and external struggles in vegetable life.

I will now proceed to consider animal life, and in this case I will begin with the internal life of animals, which is a continual struggle. That great pump the heart is continuously beating—that is, conquering resistance. It is forcing the blood through the arteries, they assisting in squeezing it onwards. If they give way, the animal dies; if they become rigid and resist too much, the animal dies. There must be a regulated antagonism, a rhythmical pulsation, the very term involving force and resistance. That the act of breathing is antagonistic scarcely needs argument. The muscular action by which the ribs are made to open out and close alternately, in order to inhale and exhale air, and other physiological changes which I cannot here go into, necessitate a continuous fight for life. So with digestion, assimilation, and other functions, mechanical and chemical forces and resistances come into play.

Since this lecture was written, I have heard of a discovery made, I am informed, by Prof. Metschnikoff, and which has brought to light a singular instance of internal antagonism. He is said to have proved that the white corpuscles of the blood are permanent enemies of Bacteria, and by inoculation will absorb poisonous germs; a recurrent war, as it appears, going on between them. If the corpuscle is the conqueror, the Bacteria are swallowed up, and the patient lives. If the corpuscles are vanquished, the patient dies, and the Bacteria live, at all events for a time. If the theory is founded, it affords a strong additional argument to the doctrine of internal antagonism. Possibly if there were no Bacteria, and the corpuscles had nothing to do, it would be worse for them and the animal whom they serve.

Let us now consider the external life of animals. I will take as an instance, for a reason which you will soon see, the life of a wild rabbit. It is throughout its life, except when asleep (of which more presently), using exertion, cropping grass, at war with vegetables, &c. If it gets a luxurious pasture it dies of repletion. If it gets too little it dies of inanition. To keep itself healthy it must exert itself for

its food; this, and perhaps the avoiding its enemies, gives its exercise and care, brings all its organs into use, and thus it acquires its most perfect form of life. I have witnessed this effect myself, and that is the reason why I choose the rabbit as an example. An estate in Somersetshire, which I once took temporarily, was on the slope of the Mendip Hills. The rabbits on one part of it, viz. that on the hill-side, were in perfect condition, not too fat nor too thin, sleek, active, and vigorous, and yielding to their antagonists, myself and family, excellent food. Those in the valley, where the pasturage was rich and luxuriant, were all diseased, most of them unfit for human food, and many lying dead on the fields. They had not to struggle for life, their short life was miserable and their death early, they wanted the sweet uses of adversity—that is, of antagonism.

The same story may be told of other animals. Carnivora, beasts or birds of prey, live on weaker animals; weaker animals herd together to resist, or, by better chance of warning, to escape, beasts of prey; while they, the Herbivora, in their turn are destroying vegetable organisms.

I now come to the most delicate part of my subject, viz. man (I include women of course!). Is man exempt from this continual struggle?

It is needless to say that war is antagonism. Is not peace so also, though in a different form? It is a common-place remark to say that the idle man is worn out by *ennui*, i.e. by internal antagonism. Kingsley's "Do-as-you-like" race — who were fed by a substance dropping from trees, who did no work, and who gradually degenerated until they became inferior to apes, and ultimately died out from having nothing to do, nothing to struggle with—is a caricature illustrative of the matter. That the worry of competition is nearly equivalent to the hardships and perils of military life, seems proved to me by the readiness with which military life is voluntarily undertaken, ill as it is paid. If it were well paid, half our men would be in the military or naval service, and I am not sure that we should not have regiments of Amazons! The increased risk of life or limbs and the arduous nature of the work do not prevent men belonging to all classes from entering these services, little remunerative as they are. Others take the risks of travelling in the deserts of Africa or wintering in the polar regions, of being eaten by lions or frozen to death, of falling from a Swiss mountain or foundering in a yacht, in preference to a life of tranquillity; and sportsmen prefer the danger of endeavouring to kill an animal that can and may kill them, to shooting tame pheasants at a *battue* or partridges in a turnip-field.

Then, in what is euphemistically called a life of peace, buyer and seller, master and servant, landlord and tenant, debtor and creditor, are all in a state of simmering antagonism; and the inventions and so-called improvements of applied science and art do not lessen it. Exercise is antagonism; at each step force is used to lift up our bodies and push back the earth; as the eminent Joseph Montgolfier said, that when he saw a company dancing, he mentally inverted his

view and imagined the earth dancing on the dancers' feet, which it most unquestionably did. Indeed, his great invention of balloons was guessed at by his witnessing a mild form of antagonism between heat and gravitation. He, being a dutiful husband, was airing his wife's dresses, who was going to a ball. He observed the hot air from the fire inflated the light materials, which rose up in a sort of spheroidal form (you may some of you have noticed this form in dress !). This gave him the idea of the fire-balloon, which, being a large paper-maker at Annonay, he forthwith experimented on, and hence we got aërial navigation. This anecdote was told me by his nephew M. Seguin, also an eminent man. Even what we call a natural death is a greater struggle than that which other animals go through, and is, in fact, the most artificial of all deaths. The lower animals, practically speaking, do experience a natural death, i. e. a violent or unforeseen death. As soon as their powers decline to such an extent that they cannot take part in the struggle for existence, they die or are killed, generally quickly, and their sufferings are not protracted by the artificial tortures arising from the endeavours to prolong life.

Let us now pass from individuals to communities. Is there less antagonism now than of yore ? Do the nations of Europe now form a happy family ? Are the armaments of Continental nations, or is the navy of this country, less than in former years ? The very expression "the Great Powers" involves antagonism.

As with wars and revolutions, so, as I have said, with regard to individuals, during our so-called peace, the fight is continuous among communities. If the water does not boil, it simmers. Not merely are there the struggles of poor against rich going on, but the battles for position and pre-eminence are constant. The subjugated party or sect seeks first for toleration, then for equalisation, and then for domination.

We call contentment a virtue, but we inculcate discontent. A father reproaches his son for not exerting himself to improve his position, and at school and college and in subsequent periods of life efforts at advancement in the social scale are recommended. Individual antagonisms, class antagonisms, political, trading, and religious antagonisms take the place of war. Can war exhibit a more vigorous and persistent antagonism than competition does ? Take the college student with ruined health ; take the bankrupt tradesman with ruined family ; take the aspirants of fashion turning night into day, and preferring gas or electric light to that of the sun : there is, to be sure, some excuse for this, as we so rarely see the latter.

But our very amusements are of a combative character : chess, whist, billiards, racing, cricket, football, &c. And in all these we, in common parlance, speak of *beating* our opponent. Even dancing is probably a relic and reminiscence of war, and some of its forms are of a military character. I can call to mind only one game which is not combative, and that is the game you are in some sort now playing, viz. "patience," and with, I fear, some degree of internal antagonism !

Take, again, the ordinary incidents of a day's life in London :

15,000 to 20,000 cabs, omnibuses, vans, private carriages, &c., all struggling, the horses pushing the earth back and themselves forwards, the pedestrians doing the same, but the horses compulsorily—they have not as yet got votes. The occupants of the cabs, vans, &c., are supposed to act from free will, but in the majority of cases they are as much driven as the horses. Insolvents trying to renew bills, rich men trying to save what they have got by saving half an hour of time. Imagine, if you can, the friction of all this, and add the bargaining in shops, the mental efforts in counting-houses, banks, &c., and road repair, now a permanent and continuous institution. Take our railways: similar efforts and resistances. Drivers, signal-men, porters, &c., and the force emanating from the sun millions of years ago, and locked up in the coal-fields, as Stephenson suggested, now employed to overcome the inertia of trains and to make them push the earth in this or that direction, and themselves along its surface. Take the daily struggles in commerce, law, professions, and legislation, and sometimes even in science and literature. Politics I cannot enter upon here, but must leave you to judge whether there is not some degree of antagonism in this pursuit. In all this there is plenty of useful antagonism, plenty of useless—much to please Ormuzd and much to delight Ahriman; but of the two extremes, over-work or stagnation, the latter would, I think, do Ahriman's work more efficiently than the former. We cry peace when there is no peace. Would the world, however, be better if it were otherwise? Is the Nirvana a pleasing prospect? Sleep, though not without its troubles and internal antagonism, is our nearest approach to it, but we should hardly wish to be always asleep.

Shakespeare not only knew something about gravitation, but he also knew something about antagonism. He says, by the mouth of Agamemnon—

> "Sith every action that hath gone before,
> Whereof we have record, trial did draw,
> Bias and thwart, not answering the aim,
> And that unbodied figure of the thought
> That gav't surmised shape."

In no case is the friction of life shown more than in the performance of "duty," i. e. an act of self-resistance, a word very commonly used; but the realisation of it is by no means so frequent. Indeed, faith in its performance so yields to scepticism that it is said that when a man talks of doing his duty, he is meditating some knavish trick.

The words good and evil are correlative: they are like height and depth, parent and offspring. You cannot, as far as I can see, conceive the existence of the one without involving the conception of the other. In their common acceptation they represent the antagonism between what is agreeable or beneficial and what is painful or injurious.

An old anecdote will give us the notion of good and evil in a

slenderly educated mind. A missionary having considered that he had successfully inculcated good principles in the mind of a previously untutored savage, produced him for exhibition before a select audience, and began his catechism by asking him the nature of good and evil. "Evil," the pupil answered, "is when other man takes my wife." "Right," said the missionary, "now give me an example of good." The answer was: "Good is when me takes other man's wife." The answer was not exactly what was expected, but was not far in disaccord with modern views among ourselves and other so-called civilised races. I don't mean as to running away with other men's wives! But we still view good and evil very much as affecting our own interests. At the commencement of a war each of the opposing parties view victory—i. e. the destruction of their enemies—as good, and being vanquished as evil. Congregations pray for this. Statesmen invoke the God of battles. Those among you who are old enough will call to mind the Crimean war. Each combatant nation gives thanks for the destruction of the enemy, each side possibly believing that they respectively are in the right, but in reality not troubling themselves much about that minor question. We (unconsciously perhaps) "compound for sins we are inclined to, by damning those we have no mind to." So in the daily life of what is called peace. The stage-coach proprietor rejoiced when he had driven his rival off the road, railway directors and shareholders now do the same, so do publicans, shopkeepers, and other rivals. We are still permeated by the old notion of good and evil. But "antagonism," as I view it, not only comprehends the relation of good and evil, but, as I have said, produces both, and is as necessary to good as to evil. Without it there would be neither good nor evil.

Judging of the lives of our progenitors from what we see of the present races of men of less cerebral development, we may characterize them as having been more impulsive than ourselves, and as having their joys and sorrows more quickly alternated. After the hunt for food, accompanied by privation and suffering, comes the feast to gorging. Their main evil was starvation, their good repletion. Even now the Esquimaux watches a seal-hole in the bitter cold for hours and days, and his compensation is the spearing and eating the seal. The good is resultant upon and in the long run I suppose, equivalent to the evil. These men look not back into the past, and forward into the future as we do. We, by extending our thought over a wider area, are led to more continuing sacrifices, and aim at more lasting enjoyment in the result. The child suffers at school in order that his future life may be more prosperous. The man spends the best part of his life in arduous toil, physical or mental, in order that he may not want in his later years, or that his family may reap the benefit of his labour. Further-seeing men spend their whole lives on work little remunerative that succeeding generations may be benefited. The prudent man transmits

health and wealth to his descendents, the improvident man poverty or gout. One main element of what we call civilisation is the capability of looking further back into the past, and further forward into the future; but, though measured on a different scale, the average antagonism and approximate equivalence appear to me to be the same.

Can we suppose a state of things either in the inorganic or the organic world which, consistently with our experience or any deduction drawn from it, would be without antagonism? In the inorganic world it would be the absence of all movement, or, what practically amounts to the same thing, movement of everything in the same direction, and the same relative velocity; for, as movement is only known to us by relation, movement where nothing is stationary or moving in a different direction or with a different velocity would be unrecognizable.

So in the organic but non-sentient world, if there were no struggle, no absorption of food, no growth, nothing to overcome, there would be nothing to call life. If, again, in the sentient world there were no appetites, no hopes—for both these involve discontent—no fear, no good or bad, what would life be? If fully carried out, is not life without antagonism no life at all, a barren metaphysical conception of existence, or rather alleged conception, for we cannot present to the mind the form of such conception.

In the most ordinary actions, such as are necessary to sustain existence, we find, as I have already pointed out, a struggle more or less intense, but we also find a reciprocal interdependence of effort and result. The graminivorous animal is during his waking hours always at work, always making a small but continuous effort, selecting his pastures, cropping vegetables, avoiding enemies, &c. The Carnivora suffer more in their normal existence; their hunger is greater, and their physical exertion when they are driven by hunger to make efforts to obtain food is more violent than with the Herbivora, if they capture their prey by speed or battle, or their mental efforts are greater if they capture it by craft. But then their gratification is also more intense, and thus there is a sort of rough equation between their pain and their pleasure, the more sustained the labour the more permanent is the gratification.

As, with food or exercise, deficiency is as injurious in one, as is excess in another direction, so as affecting the mind of communities, as I have stated it to be with individuals, the effect of a life of ease and too much repose is as much to be avoided as a life of unremitting toil. The Pitcairn islanders, who managed in some way to adapt their wants to their supply and to avoid undue increase of population, are said never to have reached old age. In consequence of the uneventful, unexcited lives they led, they died of inaction, not from deficiency of food or shelter, but of excitement. They should have migrated to England! They died as hares do when their ears are stuffed with cotton, i. e. from want of anxiety. We have hope in our

suffering, and in the mid gush of our pleasure something bitter surges up.

> "We look before and after, and pine for what is not,
> Our sincerest laughter with some pain is fraught,
> Our sweetest songs are those which tell of saddest thought."

The question may possibly occur to you, have we more or less antagonism now than in former times? We certainly have more complexity, more differentiation, in our mental characteristics, and probably in our physical, so far as the structure of the brain is concerned; but is there less antagonism? With greater complexity come increased wants, more continuous cares. Higher cerebral development is accompanied with greater nervous irritability, with greater social intricacies—we have more frequent petty annoyances, and they affect us more. With all our so-called social improvements, is there not the same struggle between crime and its repression? If we have no longer highway robberies, how many more cases of fraud exist, most of it not touched by our criminal laws? As to litigation I am perhaps not an impartial judge, but it seems to me that if law were as cheap as is desired, every next-door neighbour would be in litigation. It would seem as if social order had never more than the turn of the scale which is necessary to social existence in its favour when contrasted with the disorganizing forces. Without that there would be perpetual insurrections and anarchy. But though antagonism takes a different form it is still there. Are wars more regulated by justice than of yore? I venture to doubt it, though probably many may disagree with me. National self-interest or self-aggrandisement is, I think, the predominant factor, and is frequently admittedly so. I also doubt if the old maxim, "If you wish for peace prepare for war," is of much value. Large armaments and improvements in the means of destruction (whose inventors are more thought of than the discoverers of natural truths) are as frequently the cause of war as of its prevention. Are wars less sanguinary with 100-ton guns than with bows and arrows? I cannot enter into statistics on this subject, but a sensible writer who has, viz. Mr. Finlaison, came to the conclusion that wars cease now as anciently, not in the ratio of the improvements in killing implements, but from exhaustion of men or means. Wars undoubtedly occur at more distant intervals, or the human race would become extinct. Probably the largely increased competition supplies their place: we fight commercially more and militarily less. It is a sad reflection that man is almost the only animal that fights, not for food or means of life or of perpetuating its race, but from motives of the merest vanity, ambition, or passion. War is, however, not wholly evil. It developes noble qualities—courage, endurance, self-sacrifice, friendship, &c.—and tends to get rid of the silly encumbrances of fashion and ostentation. But do the much bepraised inventions of peace bring less antagonism? Consider the enormous labour and waste of time due to competition in the advertising system

alone. Paper-making, type-founding, printing, pasting, posting or otherwise circulating, sandwich-men, &c., all at work for purposes which, I venture to think, are in great part useless; and those who might add to the productiveness of the earth, or to the enriching our knowledge, are helping to extend the limits of the black country, and wasting their time in interested self-laudation. And the consumer pays the costs. "Buy my clothing, which will never wear out." "Become a shareholder in our company, which will pay cent. per cent." "Take my pills, which will cure all diseases," &c. These eulogies come from those highly impartial persons the advertisers, all promising golden rewards, but, as with the alchemists, on condition that gold be paid in advance for their wares; and the silly portion of the public (no small body) take them at their word. Though you may not fully agree with this my anathema of the advertising system, and though there may be some modicum of good in it, I think you will agree that it affords a notable illustration of antagonism. If I were a younger man, I think I should go to Kamchatka to avoid the penny post; possibly I should not be satisfied when I got there.

Civilisation begins by supplying wants, and ends by creating them, and each supply for the newly created want begets other wants, and so on, "*toties quoties.*" As far as we can judge by its present progress, mankind seems tending to an automatic state. The requirements of each day are becoming so numerous as to occupy the greater portion of that day; and when telegrams, telephones, electro-motion, and numerous other innovations which will probably follow these, reach their full development, no time will be left for thought, repose, or any spontaneous individual action. In this mechanical state of existence in times of peace, extremes of joy and sorrow, of good and evil, will become more rare, and the necessary uniformity of life will reduce passion and feeling to a continuous petty friction. The converse of the existence contemplated by the Stoics will be attained, and instead of a life of calm contemplation, our successors will have a life of objectless activity. The end will be swallowed up in the means. It will be all pursuit and no attainment. Is there a *juste milieu*, a point at which the superfluous *commoda vitæ* will cease? None probably would agree at where that point should be fixed, and the future alone can show whether the human race will emancipate itself from being, like Frankenstein, the slave of the monster it has created.

In the cases I have given as illustrations—and many more might be adduced—the evil resulting from apparently beneficial changes is not a mere accident: it is as necessary a consequence as reaction is a consequence of action. In the struggle for existence or supremacy inevitable in all social growths, the invention, enactment, &c., intended to remedy an assumed evil will be taken advantage of by those for whom it is not intended; the real grievance will have been exaggerated by those having an interest in trading on it, and the remedy itself will have collateral results not contemplated by those who introduce the change. I could give many instances

of this by my own experience as an advocate and judge, but this would lead me away from my subject. Evils, indeed, result from the very change of habit induced by the alleged improvement. The carriage, which saves fatigue, induces listlessness, and tends to prevent healthy exercise. The knife and fork save the labour of mastication, but by their use there is not the same stimulus to the salivary glands, not the full healthy amount of secretion, whereby digestion suffers; there is not the same exercise of the teeth whereby they are strengthened and uniformly worn, as we see in ancient skulls. It seems not improbable that their premature decay in civilised nations is due to the want of their normal exercise by the substitution of the knife and fork and stew-pan. According to the evolution theory, our organs have grown into what they are by long use, and the remission of this tends to irregular development, or atrophy. Every artificial appliance renders nugatory some pre-existing mode of action, either voluntary or involuntary; and as the parts of the whole organism have become correlated, each part being modified by the functions and actions of the others, every part suffers more or less when the mode of action of any one part is changed. So with the social structure, the same correlation of its constituent parts is a necessary consequence of its growth, and the change of one part affects the well-being of other parts. All change, to be healthy, must be extremely slow, the defect struggling with the remedy through countless but infinitesimally minute gradations.

Lastly, do the forms of government give us any firm ground to rest upon as to there being less undue antagonism in one than in another form? Whether it is better to run a risk of, say, one chance in a thousand or more of being decapitated unjustly by a despot, or to have what one may eat or drink, or whom one may marry, decided by a majority of parish voters, is a question on which opinions may differ, but there is abundant antagonism in either case.

Communism, the dream of enthusiasts, offers little prospect of ease. It involves an unstable equilibrium, i.e. it consists of a chain of connection where a defect in one link can destroy the working of the whole system, and why the executive in that system should be more perfect than in others I never have been able to see. Antagonism, on the other hand, tends to stability. Each man working for his own interests helps to supply the wants of others, thus ministering to public convenience and order, and if one or more fail the general weal is not imperilled.

You may ask, Why this universal antagonism? My answer is, I don't know; Science deals only with the How? not with the Why? Why does matter gravitate to other matter, with a force inversely as the square of the distance? Why does oxygen unite with hydrogen? All I can say is that antagonism is, to my mind, universal, and will, I believe, some day be considered as much a law as the law of gravitation. If matter is, as we believe, everywhere, even in the interplanetary spaces, and if it attracts and moves other matter, which it

apparently must do, there must be friction or antagonism of some kind. So with organized beings, Nature only recognizes the right, or rather the power, of the strongest. If twenty men be wrecked on a secluded island which will only support ten, which ten have a right to the produce of the island? Nature gives no voice, and the strongest take it. You may further ask me, *Cui bono?* what is the use of this disquisition? I should answer, If the views be true, it is always useful to know the truth. The greatest discoveries have appeared useless at the time. Kepler's discovery of the relations of the planetary movements appeared of no use at the time; no one would now pronounce it useless. I can, however, see much probable utility in the doctrine I have advocated. The conviction of the necessity of antagonism, and that without it there would be no light, heat, electricity, or life, may teach us (assuming free will) to measure effort by the probable result and to estimate the degree of probability. It may teach us not to waste our powers on fruitless objects, but to utilise and regulate this necessity of existence; for, if my views are correct, too much or too little is bad, and a due proportion is good (like many other useful things, it is best in moderation), to accept it rather as a boon than a bane, and to know that we cannot do good without effort—that is, without some suffering.

I have spoken of antagonism as pervading the universe. Is there, you may ask, any limit in point of time or space to force? If there be so, there must be a limit to antagonism. It is said that heat tends to dissipate itself, and all things necessarily to acquire a uniform temperature. This would in time tend practically, though not absolutely, to the annihilation of force and to universal death; but if there be evidence of this in our solar system and what we know of some parts of the universe, which probably is but little, is there no conceivable means of reaction or regeneration of active heat? There is some evidence of a probable zero of temperature for gases as we know them, i. e. a temperature so low that at it matter could not exist in a gaseous form; but passing over gases and liquids, if matter becomes solid by loss of heat, such solid matter would coalesce, masses would be formed, these would gravitate to each other, and come into collision. It would be the nebular hypothesis over again. Condensations and collisions would again generate heat; and so on *ad infinitum.*

Collisions in the visible universe are probably more frequent than is usually supposed. New nebulæ appear where there were none before, as recently in the constellation of Andromeda. Mr. Lockyer, as I have said, considers that they are constant in the nebulæ; and if there be such a number of meteorites as are stated to fall daily into the atmosphere of this insignificant planet, what numbers must there be in the universe? There must be a sort of fog of meteorites, and this may account, coupled with possibly some dissipation of light or change of it into other forces, for the smaller degree of light than would be expected if the universe of stellar bodies were infinite.

For if so, and the stars are assumed to be of an equal average brightness, then if there be no loss or obstruction, as light from a star decreases as the square of the distance and would from an infinite number of stars probably increase in the same ratio, the night would be as brightly illuminated as the day. We are told that there are stars of different ages—nascent, adolescent, mature, decaying, and dying; and when some of them, like nations at war, are broken up by collision into fragments or resolved into vapour, the particles fight as individuals do, and like them end by coalescing and forming new suns and planets. As the comparatively few people who die in London to-night do not affect us here, so in the visible universe one sun or planet in a billion or more may die every century and not be missed, while another is being slowly born out of a nebula. Thus worlds may be regenerated by antagonism without having for the time more effect upon the Kosmos than the people now dying in London have upon us. I do not venture to say that these collisions are in themselves sufficient to renew solar life; time may give us more information. There may be other modes of regeneration or renewed activity of the dissipated force, and some of a molecular character. The conversion of heat into atomic force has been suggested by Mr. Crookes. I give no opinion on that, but I humbly venture to doubt the mortality of the universe.

Again, is the universe limited? and if so, by what? Not, I presume, by a stone wall! or if so, where does the wall end? Is space limited, and how? If space be unlimited and the universe of suns, planets, &c., limited, then the visible universe becomes a luminous speck in an infinity of dark vacuous space, and the gases, or at all events the so-called ether, unless limited in elasticity, would expand into this vacuum—a limited quantity of ether into an infinite vacuum! If the universe of matter be unlimited in space, then the cooling down may be unlimited in time. But these are perhaps fruitless speculations. We cannot comprehend infinity, neither can we conceive a limitation to it. I must once more quote Shakespeare, and say in his words, "It is past the infinite of thought." But whatever be the case with some stars and planets, I cannot bring myself to believe in a dead universe surrounded by a dark ocean of frozen ether.

Most of you have read 'Wonderland,' and may recollect that after the Duchess has uttered some ponderous and enigmatical apophthegms, Alice says, "Oh!" "Ah," says the Duchess, "I could say a good deal more if I chose." So could I; but my relentless antagonist opposite (the clock) warns me, and I will only add one more word, which you will be glad to hear, and that word is—Finis.

[W. R. G.]

WEEKLY EVENING MEETING,

Friday, April 27, 1888.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

James Wimshurst, Esq. *M.R.I.*

Electrical Influence Machines.

I have the honour this evening of addressing a few remarks to you upon the subject of influence machines, and the manner in which I propose to treat the subject is to state as shortly as possible, first, the historical portion, and afterwards to point out the prominent characteristics of the later and more commonly known machines. The diagrams upon the screen will assist the eye to the general form of the typical machines, but I fear that want of time will prevent me from explaining each of them.

In 1762 Wilcke described a simple apparatus which produced electrical charges by influence, or induction, and following this the great Italian scientist, Alexander Volta, in 1775 gave the electrophorus the form which it retains to the present day. This apparatus may be viewed as containing the germ of the principle of all influence machines yet constructed.

Another step in the development was the invention of the doubler by Bennet in 1786. He constructed metal plates which were thickly varnished, and were supported by insulating handles, and which were manipulated so as to increase a small initial charge. It may be better for me to here explain the process of building up an increased charge by electrical influence, for the same principle holds in all of the many forms of influence machines.

This Volta electrophorus, and these three blackboards, will serve for the purpose. I first excite the electrophorus in the usual manner, and you see that it then influences a charge in its top plate; the charge in the resinous compound is known as negative, while the charge induced in its top plate is known as positive. I now show you by this electroscope that these charges are unlike in character. Both charges are, however, small, and Bennet used the following system to increase them.

Let these three boards represent Bennet's three plates. To plate No. 1 he imparted a positive charge, and with it he induced a negative charge in plate No. 2. Then with plate No. 2 he induced a positive charge in plate No. 3. He then placed the plates Nos. 1 and 3 together, by which combination he had two positive charges within practically the same space, and with these two charges he induced a

double charge in plate No. 2. This process was continued until the desired degree of increase was obtained. I will not go through the process of actually building up a charge by such means, for it would take more time than I can spare.

In 1787 Carvallo discovered the very important fact, that metal plates when insulated, always acquire slight charges of electricity; following up those two important discoveries of Bennet and Carvallo, Nicholson in 1788 constructed an apparatus, having two discs of metal insulated and fixed in the same plane. Then by means of a spindle and handle, a third disc, also insulated, was made to revolve near to the two fixed discs, metallic touches being fixed in suitable positions. With this apparatus he found that small residual charges might readily be increased. It is in this simple apparatus that we have the parent of influence machines (Diagram 1), and as it is now a hundred years since Nicholson described this machine in the Phil. Trans., I think it well worth showing a large sized Nicholson machine at work to-night.

In 1823 Ronalds described a machine in which the moving disc was attached to and worked by the pendulum of a clock. It was a modification of Nicholson's doubler, and he used it to supply electricity for telegraph working. For some years after these machines were invented no important advance appears to have been made, and I think this may be attributed to the great discoveries in galvanic electricity which were made about the commencement of this century by Galvani and Volta, followed in 1831 to 1857 by the magnificent discoveries of Faraday in electro-magnetism, electro-chemistry, and electro-optics, and no real improvement was made in influence machines till 1860, in which year Varley patented a form of machine shown in Diagram 2. It also was designed for telegraph working.

In 1865 the subject was taken up with vigour in Germany by Toepler, Holtz, and other eminent men. The most prominent of the machines made by them are figured in the Diagrams 3 to 6, but time will not admit of my giving an explanation of the many points of interest in them; it being my wish to show you at work such of the machines as I may be able, and to make some observations upon them.

In 1866 Bertsch invented a machine, but not of the multiplying type; and in 1867 Sir William Thomson invented the form of machine shown in Diagram 7, which, for the purpose of maintaining a constant potential in a Leyden jar, is exceedingly useful.

The Carré machine was invented in 1868, and in 1880 the Voss machine was introduced, since which time the latter has found a place in many laboratories. It closely resembles the Varley machine in appearance, and the Toepler machine in construction.

In condensing this part of my subject, I have had to omit many prominent names and much interesting subject matter, but I must state that in placing what I have before you, many of my scientific friends have been ready to help and to contribute, and, as an instance

of this, I may mention that Professor Sylvanus P. Thompson at once placed all his literature and even his private notes of reference at my service.

I will now endeavour to point out the more prominent features of the influence machines which I have present, and, in doing so, I must ask a moment's leave from the subject of my lecture to show you a small machine made by that eminent worker, Faraday, which, apart from its value as his handiwork, so closely brings us face to face with the imperfect apparatus with which he and others of his day made their valuable researches.

The next machine which I take is a Holtz. It has one plate revolving, the second plate being fixed. The fixed plate, as you see, is so much cut away, that it is very liable to breakage. Paper inductors are fixed upon the back of it, while opposite the inductors, and in front of the revolving plate, are combs. To work the machine (1) a specially dry atmosphere is required; (2) an initial charge is necessary; (3) when at work the amount of electricity passing through the terminals is great; (4) the direction of the current is apt to reverse; (5) when the terminals are opened beyond the sparking distance the excitement rapidly dies away; (6) it does not part with free electricity from either of the terminals singly.

It has no metal on the revolving plates, nor any metal contacts; the electricity is collected by combs which take the place of brushes, and it is the break in the connection of this circuit which supplies a current for external use. On this point I cannot do better than quote an extract from page 339 of Sir William Thomson's Papers on Electrostatics and Magnetism, which runs: "Holtz's now celebrated electric machine, which is closely analogous in principle to Varley's of 1860, is, I believe, a descendant of Nicholson's. Its great power depends upon the abolition by Holtz of metallic carriers and metallic make-and-break contacts. It differs from Varley's and mine by leaving the inductors to themselves, and using the current in the connecting arc."

In respect to the second form of Holtz machine (Fig. 4) I have very little information, for since it was brought to my notice nearly six years ago I have not been able to find either one of the machines or any person who had seen one. As will be seen by the diagram it has two discs revolving in opposite directions, it has no metal sectors and no metal contacts. The "connecting arc circuit" is used for the terminal circuit. Altogether I can very well understand and fully appreciate the statement made by Professor Holtz in '*Uppenborn's Journal*' of May 1881, wherein he writes "that for the purpose of demonstration I would rather be without such machines."

The first type of Holtz machine has now in many instances been made up in multiple form, within suitably constructed glass cases, but when so made up great difficulty has been found in keeping each of the many plates to a like excitement. When differently excited the one set of plates furnished positive electricity to the comb, while

the next set of plates gave negative electricity—as a consequence no electricity passed the terminals.

To overcome this objection, to dispense with the dangerously cut plates, and also to better neutralise the revolving plate, throughout its whole diameter, I made a large machine having twelve discs 2 feet 7 inches in diameter, and in it I inserted plain rectangular slips of glass between the discs, which might readily be removed; these slips carried the paper inductors. To keep all the paper inductors on one side of the machine to a like excitement, I connected them together by a metal wire. The machine so made worked splendidly, and your late secretary, Mr. Spottiswoode, sent on two occasions to take note of my successful modifications. The machine is now ten years old, but still works splendidly. I will show you a smaller sized one at work.

The next machine on which I make observations, is the Carré. It consists essentially of a disc of glass which is free to revolve without touch or friction. At one end of a diameter it moves near to the excited plate of a frictional machine, while at the opposite end of the diameter is a strip of insulating material, opposite which, and also opposite the excited amalgam plate, are combs for conducting the induced charges, and to which the terminals are metallically connected; the machine works well in ordinary atmosphere, and certainly is in many ways to be preferred to the simple frictional machine. In my experiments with it I found that the quantity of electricity might be more than doubled by adding a segment of glass between the amalgam cushions and the revolving plate. The current in this type of machine is constant.

The Voss machine has one fixed plate and one revolving plate. Upon the fixed plate are two inductors, while on the revolving plate are six circular carriers. Two brushes receive the first portions of the induced charges from the carriers, which portions are conveyed to the inductors. The combs collect the remaining portion of the induced charge for use as an outer circuit, while the metal rod with its two brushes neutralises the plate surface in a line of its diagonal diameter. When at work it supplies a considerable amount of electricity. It is self-exciting in ordinary dry atmosphere It freely parts with its electricity from either terminal, but when so used the current frequently changes its direction, hence there is no certainty that a full charge has been obtained, nor whether the charge is of positive or negative electricity.

I next come to the type of machine with which I am more closely associated, and I may preface my remarks by adding that the invention sprang solely from my experience gained by constantly using and experimenting with the many electrical machines which I possessed. It was from these I formed a working hypothesis which led me to make the small machine now before you. The machine is unaltered. It excited itself when new with the first revolution. It so fully satisfied me with its performance that I had four others

made, the first of which I presented to this Institution. Its construction is of the simplest character. The two discs of glass revolve near to each other, and in opposite directions. Each disc carries metallic sectors; each disc has its two brushes supported by metal rods, the rods to the two plates forming an angle of 90° with each other. The external circuit is independent of the brushes, and is formed by the combs and terminals.

The machine is self-exciting under all conditions of atmosphere, owing probably to each plate being influenced by, and influencing in turn its neighbour, hence there is the minimum surface for leakage. When excited the direction of the current never changes; this circumstance is due probably to the circuit of the metallic sectors and the make-and-break contacts always being closed, while the combs and the external circuit are supplemental, and for external use only. The quantity of electricity is very large and the potential high. When suitably arranged the length of spark produced is equal to nearly the radius of the disc. I have made them from 2 inches to 7 feet in diameter, with equally satisfactory results. The Diagram No. 9 shows the distribution of the electricity upon the plate surfaces, when the machine is fully excited. The inner circle of signs corresponds with the electricity upon the front surface of the disc. The two circles of signs between the two black rings refer to the electricity between the discs, while the outer circle of signs corresponds with the electricity upon the outer surface of the back disc. The diagram is the result of experiments which I cannot very well repeat here this evening, but in support of the distribution shown on the diagram I will show you two discs at work made of a flexible material, which when driven in one direction, close together at the top and bottom, while in the horizontal diameter they are repelled. When driven in the reverse direction the opposite action takes place.

I have also experimented with the cylindrical form of the machine; the first of these I made in 1882, and it is before you. The cylinder gives inferior results to the simple discs, and is more complicated to adjust. You notice I neither use nor recommend vulcanite, and it is perhaps well to caution my hearers against the use of that material for the purpose, for it warps with age, and when left in the daylight it changes and becomes useless.

I have now only to speak of these larger machines. They are in all respects made up with the same plates, sectors, and brushes as were used by me in the first experimental machines, but for convenience sake they are fitted in numbers within a glass case.

This machine has eight plates of 2 feet 4 inches diameter; it has been in the possession of the Institution for about three years.

This large machine, which has been made for this lecture, has twelve discs, each 2 feet 6 inches in diameter. The length of spark from it is $13\frac{5}{8}$ inches.

During the construction of the machine every care was taken to avoid electrical excitement in any of its parts, and after its completion

several friends were present to witness the fitting of the brushes and the first start. When all was ready the terminals were connected to an electroscope, and the handle was moved so slowly that it occupied thirty seconds in moving one-half revolution, and at that point violent excitement appeared.

The machine has now been standing with its handle secured for about eight hours; no excitement is apparent, but still it may not be absolutely inert; of this each one present must judge, but I will connect it with this electroscope, and then move the handle slowly, so that you may see when the excitement commences and judge of its absolutely reliable behaviour as an instrument for public demonstration. I may say that I have never under any condition found this type of machine to fail in its performance.

I now propose to show you the beautiful appearances of the discharge, then the length of sparks, which appear to be almost continuous, and then in order that you may judge of the relative capabilities of each of these three machines, we will work them all at the same time.

The large frictional machine which is in use for this comparison belongs to this Institution. It was made for Napoleon in 1822, and its great power is so well known to you that a better standard could not be desired.

These five Leyden jars are of equal size; I will connect one of them only to the large frictional machine, while I connect two jars to each of the two large machines of the influence type. The difference in power of the machines is then seen to be very marked. The exhibition may be considered as a miniature thunderstorm with almost no intermission between the lightning flashes.

In conclusion I may be permitted to say that it is fortunate I had not read the opinions of Sir William Thomson and Professor Holtz, as quoted in the earlier part of my lecture, previous to my own practical experiments. For had I read such opinions from such authorities I should probably have accepted them without putting them to practical test. As the matter stands I have done those things which they said I ought not to have done, and I have left undone those things which they said I ought to have done, and by so doing I think you must freely admit, that I have produced an electric generating machine of great power, and have placed in the hands of the physicist, for the purposes of public demonstration, or original research, an instrument more reliable than anything hitherto produced.

[J. W.]

ANNUAL MEETING,

Tuesday, May 1, 1888.

Sir Frederick Bramwell, D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1887, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 81,000*l.* entirely derived from the Contributions and Donations of the Members.

Forty-one new Members were elected in 1887.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1887.

The Books and Pamphlets presented in 1887 amounted to about 283 volumes, making, with 463 volumes (including Periodicals bound) purchased by the Managers, a total of 746 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. D.C.L. LL.D.
Treasurer—Henry Pollock, Esq.
Secretary—Sir Frederick Bramwell, D.C.L. F.R.S. M. Inst. C.E.

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John Hall Gladstone, Esq. Ph.D. F.R.S.
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Rev. John Macnaught, M.A.
Sir Frederick Pollock, Bart. M.A.
William Henry Preece, Esq. F.R.S. M. Inst. C.E.
John Rae, M.D. LL.D. F.R.S.
Sir Henry Thompson, F.R.C.S.

Visitors.

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William Anderson, Esq. M. Inst. C.E.
Benjamin Baker, Esq. M. Inst. C.E.
John Birkett, Esq. F.R.C.S.
Michael Carteighe, Esq. F.C.S.
James Farmer, Esq. J.P.
John Piggin Fearfield, Esq.
Ernest H. Goold, Esq. F.Z.S.
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James Wimshurst, Esq.

WEEKLY EVENING MEETING,

Friday, May 4, 1888.

Colonel James A. Grant, C.B. C.S.I. F.R.S. Vice-President, in the Chair.

J. K. Laughton, M.A. R.N. Professor of Modern History, King's College, London.

The Invincible Armada: a Tercentenary Retrospect.

The completion of three centuries since our great victory over the Spanish fleet in the summer of 1588 has not unnaturally given rise to renewed interest in the history of our past glories, and has recalled to many minds the wholesome sentiment that "Britannia rules the waves." There is, however, some danger of misunderstanding whilst repeating the words: of thinking that if in past years Britannia ruled the waves, she did so by right Divine, or by some special and exceptional favour of Providence, rather than by the wise provisions of her Government and by the skill and discipline of her seamen. My object this evening is, therefore, not so much to retrace the glorious but often told story, as, whilst calling up the main facts to your remembrance, to lead you to examine more or less critically into the true meaning of the great event, the circumstances of which have been overlaid with a great deal of fable and of national or religious prejudice, all fatal to anything like a philosophical or scientific inquiry, which demands an equable temper and an attention to details such as the graphic historian either slurs over or considers to be beyond the scope of his researches.

I may say then, at the outset, that I conceive the religious prejudice to be entirely misplaced. That the opposing Governments did invoke the aid of religious sentiment, is true enough; so would the Governments of Russia and Turkey, for instance, at the present day; but the Elizabethan war with Spain had its origin in two perfectly clear but wholly mundane causes; the first and chief of which was the exclusive commercial policy adopted and enforced by the Spanish Government in respect of its West Indian and American settlements. That such a policy should give rise to smuggling was almost a matter of course; and amongst the smugglers were two men who, by force of character, by genius curiously well adapted to the circumstances of the age, and by undaunted courage, were destined to achieve a foremost place in the roll of English seamen. Their names were John Hawkyns and Francis Drake. In September 1568 these two men, with some few companions and a little squadron of five small vessels, after a lucrative though illicit traffic through the Spanish settlements, were caught at anchor, in the harbour of San Juan de Lua,

by a vastly superior Spanish force, and were overwhelmed. Hawkyns and Drake, in two of the smallest vessels, alone escaped.

Ordinary men, under the circumstances, would have digested their loss as they best might; but these were far indeed from being ordinary men, and they determined by fair means or foul to exact compensation for the injury which they conceived had been done them. Hawkyns entered into a simulated negotiation to hand over a considerable part of the navy of England to King Philip, on condition of having the men who had been taken prisoners set free, and of receiving money compensation for his loss. This peculiar intrigue forms an amusing episode in the history of the Ridolfi plot in 1571. Drake, on the other hand, finding compensation not forthcoming, resolved to seek it for himself; and after some preliminary cruises, made that wonderful and adventurous voyage, in which, with a mere handful of men, he took Nombre de Dios, sacked Venta Cruz, captured a convoy of mules laden with silver, and returned home with more treasure than any one ship had previously brought to England. His achievement was to be speedily surpassed, and by himself. Four years afterwards he started on a voyage for the South Sea, and, capturing Spanish ships by the score and Spanish towns by the dozen, put a girdle round the globe and returned to England, again bringing back an enormous quantity of treasure, to the amount, it was said, of a million and a half sterling. The outcry of the friends of Spain was very loud. Drake, they said, was a pirate; and unless he was punished, war with Spain was inevitable. Elizabeth had apparently made up her mind that, in any case, war was extremely probable; and to give back money on which she had once got her clutches was to her a constitutional impossibility. She kept the money, and she knighted Drake. Now we, as Englishmen, can admire the achievements of this man, and can sympathise with the wrongs which impelled him to them; but we must at the same time admit that, were we Spaniards, we might take a very different view of Drake's career. It is, at any rate, quite certain that the king of Spain, and not only the king, but every one of his subjects, considered Drake as a pirate who ought to have been hanged, and maintained that the approval and support which he received from the English crown was a distinct and valid reason for an appeal to arms.

The other and almost equally valid reason, was the countenance and assistance which had been given by the English, indirectly and directly, to the king's rebellious subjects in the Low Countries. There were, of course, many other grounds of ill-will, beginning, it may be, with Elizabeth's refusal to marry Philip. The quarrel had been growing all along: Elizabeth had seized the Duke of Alva's treasure; had allowed Dutch privateers to shelter in English harbours; had supported Dutch rebels. Philip, on the other hand, had stirred up and fomented rebellion in Ireland, and had been a party to many plots in England — plots against the Queen's sovereignty, plots against the Queen's life. The breach was by no means a one-sided one; though we are naturally accustomed to lay most stress

on our own grievances, real and sentimental. What brought matters to a climax were the embargo laid on English shipping in Spain in May 1585, and the dread of Spain, which could now only be considered as a hostile power, obtaining the command of the Dutch ports.* It is not a little curious to note how the war between the two countries, which avowedly began in 1585, anticipated the lines of the war of the French Revolution two centuries later. In both cases the immediate cause of war was the dread of a hostile power fortifying itself in the sea-ports of the Netherlands; to prevent this a levy of men was ordered; the newly-raised army was sent abroad under an incompetent general, whose sole title to command was royal favour—it matters little whether he was called Earl of Leicester or Duke of York—and the result was ignominious failure. But meantime the English fleet swept the West Indies, and Drake's expedition of 1585-6 was the precursor and prototype of Jervis's campaign of 1794. It will be seen that this correspondence was not only in the commencement of the wars, but also in their more advanced stages; that the flat-bottomed boats at Dunkirk were imitated by those at Boulogne; and that the destruction of the enemy's ships at Cadiz in 1596 presents a very exact analogy to the final overthrow of Bonaparte's schemes at Trafalgar.

Drake's brilliant raid through the West Indies determined Philip on a decided course. For the past fifteen years the invasion of England had been mooted, as a thing desirable and not impossible. It had been proposed by the Duke of Alva in 1569; and more recently, in 1583, after his victory over Strozzi and his scratch fleet—mostly of French adventurers—at Tercera, the Marquis of Santa Cruz had urged it as a necessary step towards the reduction of the rebellious Netherlands.† The Duke of Parma had written to the same effect, repeating that English soldiers were of little count in presence of the Spanish veterans, and adding a statement, which seems to have obtained general credence among the Spaniards, that the English ships at Tercera had been the first to fly; had, in fact, played a part somewhat resembling that of the Egyptian ships at Actium. It is quite possible that there were some English ships at Tercera, though it is doubtful; if there were, they certainly did not imitate Strozzi's ill-judged and suicidal manœuvre of closing with the Spaniards, and—small blame to them—effected their escape. True or not, however, it appears certain that this reported flight of the English ships did have very considerable weight with many of the king's advisers; and so advised, and at the same time impelled by wrath, he determined on the attempt. The Marquis of Santa Cruz was called on for his scheme, which extended to gigantic proportions. Everything was to

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* 'State Papers,' Domestic, clxxx. 35-40.

† 'La Armada Invencible,' por el Capitan de Navío C. Fernandez Duro, tom. i. p. 241. Many of the papers collected by Captain Duro have been published elsewhere; many others are published by him for the first time: as one easily available source, it seems more convenient to refer for all of them to his most interesting and valuable work.

be done from Spain. The whole shipping of the empire was to be collected. Every available soldier was to be mustered. According to the very detailed project submitted by Santa Cruz on 22nd March, 1586, the numbers amounted to:—

	Nos.	Tons.	Sailors.	Soldiers.
Great ships of war	150	77,250	16,612	55,000
Store ships	40	8,000		
Smaller vessels	320	25,000		

besides—

	Nos.	Sailors and Fighting Men.	Rowers.
Galeasses	6	720	1800
Galleys	40	3200	8000

giving a total of 556 ships of all kinds, and 85,332 men, to which were to be added cavalry, artillerymen, volunteers, and non-combatants, bringing up the number of men to a gross total of 94,222.*

A project so vast and so costly did not come within the king's idea of "practical politics"; he resolved on the expedition, but conceived the idea of doing it at a cheaper rate by utilising the army in the Low Countries. From this grew up the scheme which ultimately took form. The Duke of Parma was to prepare an army of invaders in the Netherlands, and a number of flat-bottomed boats to carry it across the sea. The Marquis of Santa Cruz was to bring up the Channel a fleet powerful enough to crush any possible opposition, and carrying with it a body of troops, which, when joined with those under Parma, would form an army at least as numerous as that which Santa Cruz had detailed as sufficient.

The necessary preparations were extensive; and it is not quite clear that, as they became more definite, Philip's ardour did not somewhat slacken. The cost was certain; the issue was doubtful; and even if successful, the result might perhaps not be exactly what was desired. Philip had always posed as a supporter of the Queen of Scots; but the doubt must have suggested itself whether it was worth while, at this great cost, to conquer a kingdom for her; a kingdom which, with her French blood and French proclivities, would become virtually a French province. The death of the Queen of Scots, on 8–18 February, 1587, removed this difficulty. Even if the conquered kingdom was to be handed over to James, James was not bound to France as his mother had been. Placed on the throne of England by Spanish arms, he might be expected or even constrained to hold it virtually as a Spanish fief. And then, would it be necessary to give it to James at all? Elizabeth, of course, was outside the reckoning;

* Duro, i. p. 253.

once dispossessed, she was merely the illegitimate offspring of an abominable and incestuous concubinage. James appeared to be the legitimate heir; but Philip himself was lineally descended from John of Gaunt, and had a theoretical claim to the throne of England distinctly superior to that which, in the case of Henry VII., had been held sufficient. As an abstract problem in genealogy, Philip's claim was by no means absurd. Whether it could become something more, and take a practical form, might very well depend on the fortune of war.

Preparations were therefore now hurried on in earnest. Ships were collected at the several ports, and especially at Lisbon and Cadiz. It seemed probable that the invasion would be attempted in the summer of 1587, when, some months before, Drake, with a fleet of twenty-four ships, all told, appeared on the coast. The orders under which he sailed from England on 2nd April, were to prevent the different Spanish squadrons from joining, and where he found their ships, to destroy them. It was a grand and masterful step, but it had scarcely been ordered before the Queen repented of it. Counter orders were sent post-haste to Plymouth, but Drake had already sailed. They followed him, but never found him; perhaps the bearer of them was not too eager to find him. At any rate, Drake never got these orders, and acting on those first given, with which he had sailed, he did at Cadiz "singe the king of Spain's beard" in a most effective manner. Thirty-seven ships there collected, were sunk, burnt, or brought away. They were as yet unarmed, unmanned, and, when the forts were once passed, could offer no resistance. Other damage Drake did, insulting Santa Cruz in the very port of Lisbon, offering battle, which Santa Cruz was in no position to accept. Ships he had in numbers; but they too were neither manned nor armed; nor had he arms for them; and though, with Drake off the mouth of the Tagus, the happy thought occurred to the authorities on shore to melt down the church bells, and make guns to drive him away, before the guns were ready Drake had stretched off to the Azores, where he captured the *San Philip*, a very large and rich East Indiaman, whose treasures are said to have first opened the eyes of our English merchants to the capabilities of Eastern trade, and to have led to the foundation of the East India Company.

The destruction of shipping and stores at Cadiz necessarily delayed the equipment of the Spanish fleet; the year passed away, and it was not ready. The following February (1588) the Marquis of Santa Cruz died. The loss to Spain was incalculable, for he was the only man who by birth was entitled, and by experience was competent, to command such an expedition as that which he had set on foot. His name was encircled with a halo of naval victory. He had held a high command at the battle of Lepanto; and in the action at Tercera was accredited with having put to ignominious flight these very English who were now the object of attack. Curiously, however, the king and his court do not seem to have realised their loss, and with a light heart appointed Don Alonso Perez de Guzman el

Bueno, Duke of Medina-Sidonia, to the vacant command. Medina-Sidonia, now in his thirty-eighth year, was a man with no qualification for the post except his distinguished birth and a gentleness of temper which, it was perhaps thought, would fit better with the idea of making him subordinate to the Duke of Parma. It had indeed appeared that Santa Cruz was not in the least disposed to accept this inferior part; and it may very well be that the king was almost relieved by the solution of the difficulty which his death had offered. His successor was utterly ignorant of naval affairs, had but little experience of military, and none whatever of high command. Personally brave, as became his long line of ancestry, he was, as a commander, by his total want of experience and knowledge, timid, undecided, and vacillating. His answer to the king on being ordered to take on himself the command is, in itself, a curiosity. The business, he wrote, was so great, so important, that he could not conscientiously undertake it, being, as he was, altogether without experience or knowledge of either the sea or of war.* His objections were, however, overruled; and in an evil hour for his reputation, he consented. The equipment of the fleet was pushed on, and by the middle of May it was ready to sail from the Tagus. It did actually sail on 20–30 May.

I may here say that the name "Invincible," so commonly given to this fleet, was certainly not official. I know that, in common belief, it was given to it by the king himself. In Philip's numerous letters there is no trace of any such thing. By him, by his secretary, by Medina-Sidonia and other officers, the fleet is spoken of as the Grand Fleet—a name constantly used in England during the eighteenth century for what we would now call the Channel Fleet. In a semi-official list printed at Lisbon—a copy of which got into Burghley's hands, and is now in the British Museum—it is called "La felicissima Armada," the fortunate fleet; but the term "Invincible" is unknown. It would seem probable that the name sprung out of the idle talk of some of the young adventurers—braggarts as became their age—or out of the silly gossip of the Lisbon taverns.

None the less, however, the power and might of Spain were at this time so great, that when it was known they were being put forth to crush England, the thing was regarded as done. *Anglia fuit* was something like the expression of this general idea. Of the European opinion of the power of Spain at this epoch there is an admirable summary in the opening sentences of Lord Macaulay's 'Essay on the War of the Succession in Spain.' The Spaniard, he says, was, in the apprehension of our ancestors, "a kind of dæmon, horribly malevolent, but withal most sagacious and powerful." Their language is just such "as Arminius would have used about the Romans." "It is the language of a man burning with hatred, but cowed by those whom he hates, and painfully sensible of their superiority, not only in power, but in intelligence."

* Duro, i. p. 415.

There was, however, one class of her Majesty's subjects, the members of which had not this exalted opinion of Spanish power or of Spanish prowess. For the last twenty years English sailors had been, in their own irregular way, fighting the Spaniards on every sea where they were to be met, and had come to the conclusion that, whatever the Spaniard might be ashore, afloat he was but a poor creature: the experiences of Drake, Hawkyns, Fenton, Fenner, and a score of others whose names are less familiar, had proved that even with great apparent odds in their favour, Spaniards were not invincible. Of all the panic-stricken accounts of the great Armada which have come down to us, it is well to point out that not one was written by a seaman, or by any one who had practical knowledge of the Spaniards by sea. You are all familiar with the exaggerations of contemporary historians. The Spanish ships were so huge that ocean groaned beneath their weight; so lofty, that they resembled rather castles or fortresses; so numerous, that the sea was invisible, the spectator thought he beheld a populous town. What English sailors thought of them may be judged from a letter written by Fenner, who was with Drake when he burnt the shipping at Cadiz. "Twelve of her Majesty's ships," he said, "were a match for all the galleys in the king of Spain's dominions."

But the power of Spain, the tavern gossip and braggadocio of Lisbon, and the reports of spies who felt in honour bound to give full value for their hire, grossly exaggerated the size, the might, the armament and the equipment of the fleet as it sailed from Lisbon. Of the numbers, size, and armament I shall have to speak presently. The equipment, with which we are just now concerned, was so well arranged and so perfect, that by the time the fleet reached Cape Finisterre vast quantities of the provisions were found to be bad, putrid, fit for nothing but to be thrown overboard. The ships were short of water, probably because the casks were leaky. The ships themselves were also leaking—strained, it was said, by the heavy weather, but really from being overmasted. Several of them were with difficulty kept afloat; some were dismasted; and the distress was so general that Medina-Sidonia determined to put into Corunna to refit. This he did, but without taking any precautions to let his intention be known through the fleet. The Scilly Isles had been given out as the rendezvous in case of separation, and some dozen of the ships, finding they had lost sight of the Admiral, did accordingly go to the neighbourhood of the Scilly Isles, where they were duly seen and reported at Plymouth. Their recall, the collecting the fleet at Corunna, the refitting, the reprovisioning, all took time. The damage was so great, the number of sick so large, the season getting so advanced, that a council of war urgently recommended postponing the expedition till the next year. The king's orders were, however, imperative; and the fleet finally sailed from Corunna on the 12–22 July.

The main part of the English fleet was meantime mustered at Plymouth under the command of Lord Howard of Effingham, the

Lord-Admiral of England, with whom were Drake and Hawkyns as vice- and rear-admirals; several noblemen, including Lord Thomas Howard, the admiral's nephew; his two sons-in-law, Lord Sheffield and Sir Robert Southwell; and that quaint mixture of courtier, adventurer, and buccaneer, the Earl of Cumberland; together with many genuine sea-dogs, of whom the best known are Frobiser, Fenner, and Fenton. Large numbers of merchants' ships, levied by the Queen or by their own towns, had joined the fleet, which as it lay at Plymouth consisted of about 80 sail all told. From the time of his return from the coast of Spain in the previous summer, Drake had been urgent that he should be sent out again, with a still more powerful squadron, to repeat the blow. Hawkyns, Frobiser, Fenner—all the seamen of experience—were of the same opinion. Howard, guided by their advice, repeatedly pressed the importance of the step; but Elizabeth steadfastly refused. She hoped, perhaps, for peace; more probably, perhaps, she hoped that the war might continue to be carried on in the same cheap and desultory fashion as during the last three years and was unwilling to set Philip the example of more sustained efforts. It is difficult to believe that she was entirely hoodwinked by the negotiations carried on in Flanders and by the false protestations of the Duke of Parma. She was herself too accomplished in dissimulation to fall such an easy victim as she is commonly represented; but I think that she had persuaded herself that the preparations in Spain were merely a threat, which, however, might be converted into a terrible reality. And this seems to me to explain her ignoble conduct in the matter of supplying the ships. She believed that the Spaniards would not be the aggressors; that any extraordinary supply of ammunition was uncalled for, and provisions could be put on board from week to week. It was cheaper, she may have argued to herself, than to buy and ship a quantity of stores which would only have to be landed again and disposed of at a loss. And so, notwithstanding the prayers and entreaties of Howard and Drake, backed up by the opinion of every man of experience, no further attempt was made on the Spanish ports. It is probable enough that had Drake been permitted, he would have kindled such a blaze in the Tagus or in the harbour of Corunna, as would have effectually prevented the invasion which was now on foot.

It has been said over and over again* that the Duke of Medina-Sidonia was ordered by Philip to hug the French coast, so as to avoid the English fleet and to reach the Straits of Dover with his force intact. Nothing can well be more inaccurate. He was, on the contrary, ordered, if he met Drake near the mouth of the Channel, to fall on him and destroy him; it would be easier and more certain to destroy the English fleet piecemeal, than to allow it to collect in one. Nor do his instructions contain one word about hugging the French coast; on the contrary, they advise the Scilly Isles or the

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* Monson, in Churchill's 'Voyages,' iii. p. 149; Lediard's 'Naval History,' p. 253.

Lizard as a rendezvous, and suggest the propriety of seizing on some unfortified port in the South of England.* As a matter of fact, a position south of the Scilly Isles was given out as a rendezvous in the first instance; in the second, on sailing from Corunna, the rendezvous was Mount's Bay.†

In crossing the Bay of Biscay the Spaniards experienced bad weather, and were a good deal scattered; barely two-thirds of the fleet were in company when Medina-Sidonia sighted the Lizard on the morning of 19th July, according to the English calendar, which I shall henceforth follow. There, whilst waiting for the fleet to collect, he hoisted the royal standard at the fore—a sacred flag, containing in addition to the royal arms, the figures of Our Lord and the Blessed Virgin. Other flags there were by the score. The fleet was organised by provinces; and I am led to believe that the ships of each squadron wore the flag of its province—Andalusia, Guipuscoa, Naples, &c.; that they wore in addition the flags of the nobles and knights on board, and probably also the flag of the particular saint to which they were dedicated. But the flag which they appear to have worn in common as the flag of the empire was, strictly speaking, the Burgundian flag, which had been adopted by Spain in the time of Charles V. —white, a saltire raguled red. I may add that, amongst this great number and diversity of flags, the one flag which was not worn and could not be worn was the red and yellow ensign of the present day: this flag was not invented till the year 1785. I may here remind you, in passing, that the English flag at this time was the plain St. George's flag—white, a cross red; and this seems to have been worn by every English ship; though the *Ark*, Lord Howard's ship, flew the royal standard at the main—the lilies and lions, the relative position of which will recall to you the peculiar happiness of Macaulay's celebrated couplet:—

> "Look how the lion of the sea lifts up his ancient crown,
> And underneath his deadly paw treads the gay lilies down."

Whether any of the ships flew private or local flags is doubtful. I think it, however, not improbable that they did. Sir Oswald Brierly thinks that some of the ships may have worn, not the plain English flag, but the red cross on a Tudor ground, striped white and green, and has so shown it in some of his delightful pictures. That such a combination flag was occasionally used is possible enough, but I have not found any evidence of its having been worn as a national ensign at sea.

Whilst the Duke of Medina-Sidonia was lying to, off the Lizard, on 19th July, he was sighted by one of the English cruisers, the *Golden Hind*, commanded by Thomas Flemyng, who forthwith carried the news to the Admiral;‡ and, according to the familiar story, which I see no reason to doubt, found him, with the admirals

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* Duro, ii. p. 8. † Ibid. ii. pp. 27, 168.
‡ 'State Papers,' Domestic, ccxv. 62.

and captains of the fleet, playing bowls on the Hoe. My friend, Mr. Wright, of Plymouth, the indefatigable secretary of the Tercentenary Commemoration, has called my attenion to a pamphlet first published in 1624, in which the incident is referred to as a well-known fact.* But the common idea that Flemyng was a pirate; that he had been to sea pilfering;† that in venturing into the presence of the Lord Admiral, he risked his life, and only saved it by the importance of his news,‡ all this is contrary to well-ascertained fact. To say of Flemyng, or indeed of any seaman of that age, that he had not committed some irregularities which his enemies might stigmatise as piracy, is of course impossible; but he was not a man of ill-repute. He seems to have been a connection of Hawkyns,§ and certainly commanded the *Golden Hind*, a merchant-ship in the Queen's pay, and serving under the immediate orders of Drake.

The following day, Saturday the 20th, the Spanish fleet was collected off the Lizard and moved slowly eastwards. A council of war was held. They had learned that the English fleet was at Plymouth, and the great weight of opinion among the Spanish leaders was that they ought to attack it there. It has always been said that Medina-Sidonia was prevented from doing this by his instructions. The statement is inaccurate. The letter of his instructions distinctly permitted him to attack the English fleet; the spirit of them enjoined his doing it.‖ Fortunately he misunderstood his instructions; he conceived that he was bound to go up Channel, turning neither to the right hand nor to the left until he could effect a junction with the Duke of Parma. Had he, on the 19th, when he first learned that the English fleet was at Plymouth, crowded sail with even such ships as he had with him, he might have entered the Sound that evening. The wind was from the south-west, and the English ships, penned in between the Spaniards and the shore, would have been forced to fight hand to hand; the result might easily have been disaster. The Spaniards neglected their chance, and it never recurred; for during the Saturday the English got out of the Sound, and stretched along the coast to the westward. On Sunday morning, when the two fleets were first in presence of each other, the English were to windward, and by the weatherly qualities of their ships had no difficulty in keeping the advantage they had gained.

And now, before the fighting begins, it is time to speak of the comparative force of the opposing fleets. We have all known from our infancy that the Spanish ships, as compared with the English, were stupendous in point of size, marvellous in their strength; in

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* Morgan's 'Phœnix Britannicus,' p. 345.

† Monson, in Churchill, iii. p. 150. ‡ 'Westward Ho!'

§ Wright's 'Britain's Salamis,' p. 19.

‖ "Si topáredes al dicho Draques con la Armada á la boca del Canal de Ingalaterra, podreis en este caso envestirle, porque si están divididas sus fuerzas sería muy bueno irlos venciendo así, para que no se pudiesen juntar todas."—Duro, ii. p. 9.

guns and in number of men beyond all proportion. The numbers I give here, from the official Spanish record,* agree very well with those reported in England.†

Ships.	Tons.	Guns.	Men.
130	57,868	2,431	8,050 seamen. 18,973 soldiers. 1,382 volunteers, &c. 2,088 rowers.
		Total ..	30,493

In one point alone of this statement is the difference from the English account worth noticing. Barrow gives the number of Spanish guns as 3165. To this I shall presently recur. Meantime, I have to point out to you that these numbers refer to the fleet as it left Lisbon. They had suffered a marked decrease before the fleet left Corunna, and a still further decrease before the fleet came into the Channel. Of the ships left behind I have no account. Some, and some large ships amongst them, certainly did not come on. Some, again, appear to have parted company on the voyage; and of four galleys, from which much had been expected, one was driven ashore and wrecked near Bayonne; the other three, making very bad weather of it, returned to Spain.‡ Allowing for these losses, I think it doubtful whether even 120 ships of all sizes came into the Channel; the number of men did certainly not exceed 24,000; and in the council of war held at Corunna it was estimated as low as 22,500.§ On the other hand, the number of men borne in the English ships when all collected together, is officially given as 15,925, to which ought to be added many more who were sent off from Plymouth on 21st July, or who joined as volunteers during the passage up Channel. It is difficult to estimate the gross total as less than from 17,000 to 18,000 men.

Our idea of the size of the Spanish ships has been also somewhat exaggerated. According to Barrow: "The best of the Queen's ships placed alongside one of the first class of Spaniards would have been like a sloop-of-war by the side of a first rate." In point of tonnage, they were, in fact, the same. The largest Spaniard, the *Regazona*, of the Levant squadron, is given as of 1249 tons. The largest English ship, the *Triumph*, was of 1100 tons, and many circumstances lead me to believe that the English mode of reckoning tonnage gave a smaller result than the Spanish.|| There is no doubt, however, that

* Duro, ii. pp. 66, 83. † Barrow's 'Life of Drake,' p. 270.

‡ Duro, i. p. 65. As they did return, the popular story of David Gwynne (Lediard, p. 253) is, in its details at least, certainly fictitious.

§ Duro, ii. pp. 190, 142.

|| Other modes of reckoning tonnage adopted in the following reign gave results varying from 20 to 50 per cent. more.—'State Papers,' Domestic, ccxxxvii. 64.

the Spanish ships looked larger. Their poops and forecastles, rising tier above tier to a great height, towered far above the lower-built English. Not that the large English ships were by any means flush-decked; but they were not so high-charged as the Spanish. The difference offered a great advantage to the Spaniards in hand-to-hand fighting; it told terribly against them when their enemy refused to close; it made their ships leewardly and unmanageable in even a moderate breeze, and, added to the Spanish neglect of recent improvements in rig—notably, the introduction of the bowline—rendered them very inferior to the English in the open sea.*

And not only was there this inferiority of the ships, there was at least a corresponding inferiority of the seamen. The Spaniards were in fact, to a great extent, fair-weather sailors. Some there doubtless were who had doubled Cape Horn or the Cape of Good Hope, but by far the greater number had little experience beyond the Mediterranean, or the equable run down the trades to the West Indies. To the English, on the other hand, accustomed from boyhood to the Irish or Iceland fisheries; in manhood to the voyages to the north-west with Frobiser or Davys, or round the world with Drake, the summer gales of the Channel were, by comparison, passing trifles—things to be warded off, but not to be feared. Even if the men had been equal in quality, the Spanish ships were terribly undermanned. The seamen habitually gave place to the soldiers; the soldiers commanded; the seamen did the drudgery, and not one was borne in excess of what their soldier masters thought necessary. The absolute numbers speak for themselves, and one comparison will be sufficient. The *San Martin*, of 1000 tons, the flagship of the Duke of Medina-Sidonia, had 177 seamen and 300 soldiers. The *Ark*, of 800 tons, the flagship of Lord Howard, appears to have had something like 300 seamen and 125 soldiers.

More important, however, than even this inferiority of the Spanish ships and sailors, was the inferiority of their guns and gunners. Now here I come on to what is, I believe, to most of you new ground. You have always been accustomed to hear of the number and size of the Spanish guns. The statements to that effect are absolutely incorrect. The Spanish guns were, as a rule, small: 4-, 6-, or 10-pounders; they were comparatively few, and they were execrably worked.† The simplest way to show this is by a comparative table of armaments. It is not perfect; it is not rigidly accurate; the means to construct a perfect or accurate table do not, I fear, exist; but so far as it goes, the table on the opposite page embodies the best information attainable.

The English armaments shown in it are from a list dated 1595–99,‡ and may possibly show some improvement on the armament the ships carried in 1588. I see no reason, however, to suspect such.

* Compare Monson, in Churchill, iii. pp. 312, 319.

† Duro, ii. p. 237.

‡ 'Archæologia,' xiii. p. 27; Derrick's 'Rise and Progress of the Royal Navy,' p. 31.

I have not been able to trace the original from which this paper was printed, but I have found of the same date, 1595, estimates for the armament of three ships now in building,* the ordnance for the first-

COMPARISON OF ARMAMENTS.

Ships' Names.	Tons.	Men.	No. of Guns.	Weight of Broadside in lbs.	Description of Guns. Pounders. 60	30	24	18	12	9	6	Small Pieces.
Spanish:—												
S. Lorenzo	..	386	50	370	4	8	..	6	..	6	10	16
N. S. d. Rosario ..	1150	422	41	195	..	3	7	4	..	1	..	26
Anunciada	703	275	24	67	..	..	..	3	..	3	..	18
Sta. Maria d. Vison	666	307	18	54	..	..	..	..	6	..	..	12
English:—												
Triumph	1100	500	68	402	4	3	..	17	..	8	6	30
Ark	800	425	55	377	4	4	..	12	..	12	6	17
Nonpareil	500	250	56	264	2	3	..	7	..	8	12	24
Foresight	300	160	37	102	..	..	..	..	..	14	8	15
Tiger	200	100	22	83	..	..	..	..	..	6	14	2
Tramontana	150	70	21	52	..	..	..	..	..	..	12	9
Achates	100	60	13	36	..	..	..	..	..	6	..	7

mentioned being described as "answerable to the pieces that are in the *Mer Honour*." I therefore show here also the armament of the *Mer Honour*, as given in the paper in the 'Archæologia,' already referred to.

Ship's Name.	Tons.	No. of Guns.	Weight of Broadside in lbs.	Description of Guns. Pounders. 30	18	9	6	Small Pieces.
Mer Honour ..	800	41	281	4	15	16	4	2
Sept. 1595 ..	?	44	299	4	16	18	4	2
Oct. 1595, I. ..	?	44	282	..	20	20	4	..
Do. II. ..	?	36	222	..	16	12	8	..

Another estimate that seems entitled to credit, is that given of the armament of the *Revenge*—a ship of the same size and number of men as the *Nonpareil*, which was taken by the Spaniards in 1591, and was reported by them to have 43 brass guns; 20 on the lower deck of from 4000 to 6000 lbs. weight, and the rest of from 2000 to 3000.† The greater weights correspond to the 60- 30- or 18-pounders, the smaller to the 9- and 6-pounders.

Of the Spanish armament we cannot speak with the same absolute knowledge; but it seems admitted that the galeasses were the most

* 'State Papers,' Domestic, ccliii. 114; ccliv. 43.

† Duro, i. p. 76.

heavily armed ships in the fleet; and of these the *San Lorenzo*, which was taken off Calais, was the largest and heaviest. The report of her armament given by our people, who had possession of her for some time, corresponds fairly well with the official statement.* The *Nuestra Señora del Rosario* was the large ship captured by Drake and sent into Torbay.† Her armament is given from the official inventory taken at Torquay. She is spoken of by Duro as one of the most powerful and best ships of the fleet.‡ The other two ships do not seem distinguished in any way from others of the same size; they belonged to the Levant squadron, and are classed with the *San Juan de Sicilia* of 800 tons and 26 guns, which is spoken of as having taken a prominent part in the action of 29th July. I have not met with any account of the armament of the ships of the Portuguese squadron, including the *San Martin*, *San Felipe*, and *San Mateo*, of which all three were in the thickest of the fight, and the two last were driven on shore in a sinking state. Neither have I met with any inventory of the *Nuestra Señora de la Rosa*, the ship that was partially blown up, and was sent into Weymouth. I do not, of course, suppose that the more effective fighting ships of the fleet were armed like the *Anunciada* or *Santa Maria de Vison*; but I do believe that the armament of these is a fair representative of that of a very large proportion of ships that have been counted as effective.

I must note also, that whereas the Spanish ships of below 300 tons burden carried four or six small guns—a merely nominal armament —English ships of 200 tons carried a very respectable armament, and ships even still smaller were not altogether despicable. Of the way in which the English merchant ships were armed, we have no knowledge; but considering that the fitting them out for purposes of war was no novelty, that many of them had probably been on privateering cruises before, and that the *Pelican* or *Golden Hind*, in which Drake went round the world—a ship of nominally 100 tons— had 14 guns, I would distinctly question Barrow's judgment that, "looking at their tonnage, two-thirds of them, at least, would have been of little, if any, service, and indeed must have required uncommon vigilance to keep them out of harm's way."§ They were not, indeed, the ships that were to be found in the fore-front of the fight—no more were the *Euryalus* or *Naiad* at Trafalgar—but I see no reason to doubt that they did, in their own way, render good and efficient service.

It was not only in the number and weight of guns that the English had a great comparative advantage; they were immensely superior in the working of them. I may quote here from Captain Duro a very remarkable statement, which, however, is fully corroborated by original writers and by known facts. By the Spaniards, he says,‖

* 'State Papers,' Domestic, ccxiii. 67; Duro, i. p. 390.
† 'State Papers,' Domestic, ccxv. 671.
‡ Duro, i. p. 83*n*: "una de las más fuertes y mejores de la Armada."
§ 'Life of Drake,' p. 270.
‖ Duro, i. p. 77.

"the cannon was held to be an ignoble arm; well enough for the beginning of the fray, and to pass away the time till the moment of engaging hand to hand, that is, of boarding. Actuated by such notions, the gunners were recommended to aim high, so as to dismantle the enemy and prevent his escape; but, as a vertical stick is a difficult thing to hit, the result was that shot were expended harmlessly in the sea, or, at best, made some holes in the sails, or cut a few ropes of no great consequence." On the other hand, the gun was the weapon which the English sailors had early learned to trust to. Their practice might appear contemptible enough to an *Excellent's* gun's crew, but everything must have a beginning. With no disparts or side scales, with no aid beyond possibly a marked quoin to lay the gun horizontal, and with shot which—perhaps a good inch less in diameter than the bore of the gun—wobbled from side to side, or from top to bottom, leaving the gun at any angle that chance dictated, the hitting the object aimed at was excessively doubtful. Still, by firing a great many shot, they did manage to get home with sufficient to do a good deal of damage. The Spanish accounts, speaking of the quickness of the English fire, estimate the English expenditure of shot as about three times their own.* Captain FitzGerald has recently called attention to the possibility of a rapid fire of small guns being found in the day of battle superior to the slow fire of big guns. It is a very grave question, and one that deserves all the care which Captain FitzGerald can persuade our authorities to give it. But in the case we are now considering, the conditions were reversed; it was the heavy armament which was quick firing, the lighter guns which were slow.

There is another point which may very probably have also stood in the way of the Spanish gunners. Through the greater part of last century, the ports of Spanish line-of-battle ships were made much too small, with the idea, apparently, of keeping out the enemy's musketry shot, but with the actual result that their guns could neither be trained, depressed, or elevated. In this way was possible such an action as that between the *Glorioso*, a 70-gun ship, and the *King George*, a frigate-built privateer of 32 guns, in 1747; in which the two ships engaged broadside to broadside for several hours, without the privateer receiving any proportionate damage.† In the beginning, some such fault was general; and to a very great extent, the gun was brought to bear by the action of the helm; but it is at least probable that Spanish ships carried it to a still greater degree, and that this might, to some extent, exaggerate the badness of the Spanish gunnery practice, which was very bad indeed.

All this was quite well known to Philip, and therefore to the principal officers in the fleet before they left Lisbon. The king's instructions to Medina-Sidonia say:—"You are especially to take notice that the enemy's object will be to engage at a distance, on account of the advantage which they have from their artillery and

* Duro, ii. 377. † 'Studies in Naval History,' p. 243.

the offensive fireworks with which they will be provided; and on the other hand, the object on our side should be to close and grapple and engage hand to hand." * This perhaps may partly explain the comparatively small quantity of shot per gun provided for such a vast undertaking; a quantity so small, that, notwithstanding the slowness of their fire, they ran short even after the skirmishes in the Channel.

In estimating the opposing forces, this great superiority of the English armament must be taken into account. Of Spanish ships of 300 tons and upwards, the number that left Lisbon was officially stated as 80: but of these, 18 were rated as ships of burden (*urcas de carga*); and though they carried troops and some guns, could not be counted as effective ships of war. Of the remaining 62, many ought to be reckoned in the same category. An armament such as that of the *Anunciada* or *Sta. Maria* speaks for itself. From the number of soldiers they carried, and from their lofty poops and forecastles, such ships would be dangerous enough in a hand-to-hand fight, but were perfectly harmless as long as they were kept at a distance. But counting all these, we have the following comparison of the fleets:—

	Spanish.		English.	
	Nos.	Tons Average.	Nos.	Tons Average.
Of 300 tons and upwards	62	727	23	552
Of 200 to 300 tons	..	..	26	210
			49	

The English ships of 200 tons being included as unquestionably superior as fighting machines to many of the much larger Spanish ships.

I am dwelling on these points—to many of which I do not think sufficient attention has been paid—not as in any way detracting from the superlative merit of the Englishmen who fought and won in this great battle, but as showing that their achievement, however great, was still within the bounds of human prowess. The Spaniards of that time were among the most splendid soldiers that the world has seen; and to speak of our men engaging them and defeating them, against such tremendous odds as are commonly shown, is not to exalt our heroes, but to travesty them into paladins of impossible romance, or—in spite of abundant evidence to the contrary—to represent them and the land they defended as saved from extermination only by the direct interposition of Providence, and by a heaven-sent gale of wind.

Time will not permit me, nor do I think it necessary to describe to you in detail, the fight of that eventful week: to tell you how on Sunday morning, 21st July, the English, having gained the wind, fell

* Duro, ii. 9.

on the ships of the Spanish rear-guard, under the command of Don Juan Martinez de Recalde in the *Santa Ana*, and without permitting them to close, as they vainly tried to do, pounded them with their great guns for the space of three hours, with such effect that Recalde sent to Don Pedro de Valdes for assistance, his ship having been hulled several times, and her foremast badly wounded; how Don Pedro's ship, the *Nuestra Señora del Rosario*, in going to his assistance, fouled first one and then another of her consorts, lost her bowsprit, foremast and maintopmast, and was left by Medina-Sidonia, who conceived it to be his duty to push on to Dunkirk, even at the sacrifice of this large and powerful ship, which was taken possession of by Drake the next morning, and sent into Torbay; how another ship, the *Nuestra Señora de la Rosa*, of 945 tons, was partially blown up and was similarly left to be taken possession of by order of the Admiral, and to be sent into Weymouth; how on the Tuesday there was another sharp action off Portland, and again, a third on the Thursday off the Isle of Wight, when Recalde's ship, the *Santa Ana*, of 768 tons, received so much further damage that she left the fleet and ran herself ashore near Havre; how the English, joined, as they passed along, by many small vessels full of men, but finding their store of shot running short, were content for the next day with closely following up the Spaniards, who on Saturday afternoon anchored off Calais, whilst the English anchored about a mile to westward and to windward of them. Here Howard was joined by the "squadron of the Narrow Seas," under Lord Henry Seymour and Sir William Wynter, by the contingent of the City of London, under Nicholas Gorges, and by many private ships, bringing the number up to a gross total of nearly 200, a large proportion of which were very small, but of which, as I have already shown, 49 were effective ships of war. The Spanish numbers had been reduced by the loss of three, if not four, of their largest and best ships, and were further reduced off Calais by the loss of the *San Lorenzo*, the largest and most heavily armed of the galeasses. For on Sunday night Howard sent eight fireships in amongst the Spanish fleet: the Spaniards, panic-struck, cut their cables, and by wind and tide were swept far to leeward. In the confusion, the *San Lorenzo* damaged her rudder, and in the morning was driven ashore, and after a sharp fight, captured by the boats of the *Ark* and some of the smaller ships. But the fleet was away off Gravelines; and there on that Monday, 29th July, was fought the great battle which—more distinctly perhaps than any battle of modern times—has moulded the history of Europe; the battle which curbed the gigantic power of Spain, which shattered the Spanish prestige, and established the basis of England's Empire.

It would be pleasant to dwell on the details of this great fight: to tell you how the Spaniards, having formed themselves in a half-moon, convexity in front, were charged on the wings and centre by our fleet; on the westernmost or larboard wing by Drake, with Hawkyns, Frobiser, Fenton, Fenner, and others; in the centre by Howard and his kinsmen, with the Earl of Cumberland; and on the

starboard wing by Seymour, with Wynter and the squadron of the Narrow Seas; how the wings were driven in on their centre; how the ships, thus driven together, fouled each other, and lay a helpless and inert mass, whilst the English pounded them in comparative safety. "The fight," Wynter wrote, "continued from nine of the clock until six of the clock at night, in the which time the Spanish army bore away N.N.E. or north by east as much as they could, keeping company one with another. . . . I deliver it to your honour upon the credit of a poor gentleman, that out of my ship there was shot 500 shot of demi-cannon, culverin and demi-culverin; and when I was furthest off in discharging any of the pieces, I was not out of the shot of their harquebus, and most times within speech one of another; and surely every man did well. No doubt the slaughter and hurt they received was great, as time will discover it; and when every man was weary with labour, and our cartridges spent, and munitions wasted—I think in some altogether—we ceased, and followed the enemy." *

The subject is one that tempts to pursue it still further, but time warns me to draw to a close. It must be enough then to say that the Spaniards were terribly beaten; that two of their largest ships, ships of the crack Portugal squadron, the *San Felipe* and *San Mateo*, ran themselves ashore on the Netherlands' coast to escape foundering in the open sea. Howard says that three were sunk, and four or five driven ashore. In one case he can scarcely have been mistaken. "On the 30th," he says, "one of the enemy's great ships was espied to be in great distress by the captain [Robert Crosse] of her Majesty's ship called the *Hope*, who, being in speech of yielding unto the said captain, before they could agree on certain conditions, sank presently before their eyes." This may have been the *San Juan de Sicilia*, which was severely beaten in the fight and never returned to Spain, though it was not known how she was lost. The actual loss of life was certainly very great—how great was never known, for the pursuit of the English and the terrible passage round the west of Ireland prevented any attempt at official returns. Of the losses among the isles of Scotland and on the coast of Ireland I do not intend now to speak. It is sufficient for my purpose to say that, according to the best Spanish accounts, which, in such an overwhelming disaster, are rather mixed, about half of the original 130 got home again; some apparently by the simple process of not going farther than Corunna, some, as three of the galleys, by turning back before they crossed the Bay of Biscay.

A point of more immediate naval interest regards the statements that have been made of the wholesale death of the English seamen from starvation, or the unwholesome nature of the victuals which the Queen's shameful parsimony compelled them to eat. I am no particular admirer of Queen Elizabeth, but I do think that in this matter she has had hard measure dealt her. I daresay she was as mean in the matter of her accounts as she was in many other things; I daresay she refused to believe in the necessity of supplying pro-

* Wynter to Walsyngham, 1 August, 1588.—'State Papers,' Domestic, ccxiv. 7.

visions, or in the badness of some that were condemned; and I dare-say there was much suffering and some sickness in consequence. But the sickness that so terribly scourged our ships' companies seems to have been of the nature of typhus, and to have been busy in some of the ships, and especially in the *Elizabeth Jonas*, before the Spaniards came into the Channel.* This pestilence would, no doubt, be materially aggravated by scarcity and bad provisions, but it was primarily and chiefly due to infection from the shore and from ignorance or neglect of what we now know as sanitary laws. I may notice what seems an interesting point, that the ships commanded by the experienced old salts seem to have escaped comparatively lightly. The ships named as most heavily scourged are the *Elizabeth Jonas*, the *White Bear*, and the *Lion*, commanded by Howard's sons-in-law and nephew, men splendid in the day of battle, but of no experience in the very necessary art of keeping a ship clean and sweet. A similar infection, however, continued occasionally to scourge our ships' companies, and still more frequently and more severely French or Spanish ships' companies, till near the close of last century. In our service, at least, it is now happily almost forgotten.

Before I finish, there are two or three questions which I am sometimes asked, and on which therefore it seems not out of place that I should say a few words.

What active share had Ralegh in the repulse of the Armada? None at all, so far as I can ascertain. It is stated by Camden, whom Edwards, in his Life of Ralegh, follows, that he joined the fleet off Portland on 23rd July. I think this extremely doubtful, and the more so as Camden is certainly wrong in respect of some others whom he reports to have joined about the same time. It is, of course, possible that Ralegh went on board the *Ark*, and served as an amateur on the staff of the Lord Admiral; but his name is never once mentioned in any such connection, and I venture to say it is improbable.

What truth is there in the story that the Armada carried a large number of priests appointed as missionaries *ad fidem propagandam?* None at all. There were in the fleet 180 ecclesiastics, not an extravagant number when we remember that there were many nobles of a rank which might well entitle them to have a private chaplain, and that 50 years later it was not unusual for a French ship-of-war to have two chaplains.

What about the officers of the Inquisition? It seems to be a confused story arising out of the presence of a regularly appointed provost-marshall and staff. As to the chains and instruments of torture, I presume the story was imagined by some of the men who were on board the *San Lorenzo* off Calais, and saw there the irons used for shackling the slaves to their benches.

And now to conclude. I have described to you how 300 years

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* Howard to the Lord Treasurer, 10th August, 1588; Howard to the Council, 22nd August, 1588; Howard to the Queen, 22nd August, 1588.—'State Papers,' Domestic, ccxiv. 66; ccxv. 40, 41.

ago the country was exposed to a very great danger, which was made greater still by the uncertainty of the Government as to the proper way to meet it, and by their halting between two opinions. Had the money which was wasted on useless shore defences in England or in the equipment of troops for Flanders been spent in providing a few more *Arks* or *Triumphs*, or a larger supply of shot—though the want seems due to the very unusual expenditure and a very natural miscalculation—the victory would have been more assured. As it was, however, it seems to me that we have before us a grand experience, conveying, to those who can read it, most useful and important lessons. Although subject to the most grinding parsimony, our navy in the day of need was found equal to what was required of it. Professor Seeley tells us we had no navy worthy of the name before the days of the Commonwealth.* He is in error. We had not only a navy, but a navy which, by the care and prudence and ingenuity of its principal officers, and especially of Hawkyns, who had been for some years its comptroller, was superior to the navy which, in popular repute, was the most powerful in the world. It was no mere chance which made our large ships more handy, more weatherly, more heavily armed than those of Spain, and, above all, which had them ready when they were wanted. Is this the case at the present day? I fear not. I am misinformed if either our ships or guns are superior to those of our neighbours; it is, I am told, an open question whether the armaments of our ships are not inferior to those of possible enemies; and I am speaking not from popular gossip or newspaper paragraphs, but from official utterances in the House of Commons, when I say that several of our largest ships—the *Triumphs* or *Arks* of the present day—are lying in harbour useless for want of guns, which are not likely to be supplied for some months; or of gun fittings, discovered to be defective only when the ship is ordered for commission. Much has been said of want of money—of estimates reduced below starvation point. The want here, at least, is not of money, but of judgment and knowledge and effective supervision. It will be little excuse to the nation to attribute any possible disaster to a departmental error—to a dual control—to the outrageous fact that the supply of ships is regulated by one board and that of guns by another; the two working independent of, and sometimes at variance with, each other. But this is beyond my present purpose. I will only add that, as imitation is the sincerest form of flattery, so a readiness for action, similar to that which enabled our forefathers to achieve this great victory, will be the truest and the best commemoration of their glory. And yet I think that in this, its Tercentenary, there is a peculiar fitness in the desire to record it on a visible monument. Storied urn or animated bust cannot, indeed, call back the fleeting breath of the great dead; but they may aid in preserving their memories, a light to lighten our path and to guide our footsteps. [J. K. L.]

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* 'Expansion of England,' p. 81.

GENERAL MONTHLY MEETING,

Monday, May 7, 1888.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Sir James Crichton Browne, M.D. LL.D. F.R.S.
William Crookes, Esq. F.R.S.
Warren de la Rue, Esq. M.A. D.C.L. F.R.S.
Colonel James A. Grant, C.B. C.S.I. F.R.S.
Sir Frederick Pollock, Bart. M.A.
John Rae, M.D. LL.D. F.R.S.
Henry Pollock, Esq. Treasurer.
Sir Frederick Bramwell, D.C.L. F.R.S. Honorary Secretary.

John Hutton Balfour-Browne, Esq. Q.C.
Lady Roscoe.
John Callander Ross, Esq.
T. E. Thorpe, Esq. Ph.D. F.R.S.

were elected Members of the Royal Institution.

John Tyndall, Esq. D.C.L. LL.D. F.R.S. was elected Honorary Professor of Natural Philosophy.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. was elected Professor of Natural Philosophy.

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research:—

Brigadier-General H. Collett £10

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India. Records, Vol. XXI. Part 1. 8vo. 1888.

The Secretary of State for India—Report on Public Instruction in Bengal, 1886–7. fol. 1887.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. Vol. III. 2° Semestre, Fasc. 12, 13; Vol. IV. Fasc. 1. 8vo. 1887.

Asiatic Society of Bengal—Journal, Vol. LVI. Part I. Nos. 2, 3; Part II. Nos. 2, 3. 8vo. 1887-8.

Proceedings, 1887, Nos. 9, 10; 1888, No. 1. 8vo. 1887–8.

Asiatic Society, Royal (Bombay Branch)—Journal. Vol. XVII. No. 46. 8vo. 1887.
Astronomical Society, Royal—Monthly Notices, Vol. XLVIII. No. 5. 8vo 1888.
Aubertin, J. J. Esq. M.R.I. (the Author)—A Fight with Distances: The States, Hawaiian Islands, Canada, British Columbia, Cuba, Bahamas. 8vo. 1888.
Bassett, Alfred B. Esq. M.A. M.R.I. (the Author)—Treatise on Hydrodynamics. Vol I. 8vo. 1888.
Birkett, John, Esq. F.R.C.S. M.R.I.—A Copy of the Last Will and Testament of Thomas Guy. 12mo. 1732.
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Brymner, Douglas, Esq. (the Archivist)—Report of Canadian Archives, 1887. 8vo. 1888.
Cambridge Philosophical Society—Proceedings, Vol. VI. Part 4. 8vo. 1888.
Chemical Society—Journal for April, 1888. 8vo.
Chief Signal Officer, U.S. Army—Annual Report for 1886. 8vo.
East India Association—Journal, 1888, No. 1. 8vo.
Editors—American Journal of Science for April, 1888. 4to.
 Analyst for April, 1888. 8vo.
 Athenæum for April, 1888. 4to.
 Chemical News for April, 1888. 8vo.
 Chemist and Druggist for April, 1888. 8vo.
 Engineer for April, 1888. fol.
 Engineering for April, 1888. fol.
 Horological Journal for April, 1888. 8vo.
 Industries for April, 1888. fol.
 Iron for April, 1888. 4to.
 Murray's Magazine for April, 1888. 8vo.
 Nature for April, 1888. 4to.
 Revue Scientifique for April, 1888. 4to.
 Scientific News for April, 1888. 4to.
 Telegraphic Journal for April, 1888. 8vo.
 Zoophilist for April, 1888. 4to.
Ellis, G. B. Esq. M.R.I.—Britain's Salamis: or the Fight of 1588. By W. H. K. Wright. 8vo. 1888.
Feis, Jacob, Esq. (the Author and Translator)—Shakspere and Montaigne. 12mo. 1884.
 Locksley Hall nach sechzig Jahren. Aus dem Englischen des Lord Tennyson. 12mo. 1888.
Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 55. 8vo. 1888.
Franklin Institute—Journal, No. 748. 8vo. 1888.
Geographical Society, Royal—Proceedings, New Series, Vol X. No. 4. 8vo. 1888.
Geological Institute, Imperial, Vienna—Verhandlungen, 1887, Nos. 17–18; 1888, Nos. 1–4. 8vo.
Johns Hopkins University—American Chemical Journal, Vol. X. No. 2. 8vo. 1888.
 University Circular, No. 64. 4to. 1888.
Linnean Society—Journal, No. 139. 8vo. 1888.
Madras Government Central Museum—Coins: Catalogue No. I. Mysore. By E. Thurston. 8vo. 1888.
Madrid Royal Academy of Sciences—Anuario, 1888. 8vo.
 Memorias, Tomo XII. XIII. Parte 1ª. 4to. 1887.
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Manchester Geological Society—Transactions, Vol. XIX. Parts 16, 17. 8vo. 1888.
Mechanical Engineers' Institution—Proceedings, 1888, No. 1. 8vo.
Meteorological Office—Report of Meteorological Council, R.S. 31st March, 1887. 8vo. 1888.
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WEEKLY EVENING MEETING,

Friday, May 11, 1888.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

Professor W. C. Roberts-Austen, F.R.S. *M.R.I.*

Some curious properties of Metals and Alloys.

(Abstract deferred).

WEEKLY EVENING MEETING,

Friday, May 18, 1888.

John Rae, M.D. LL.D. F.R.S. Vice-President, in the Chair.

M. Alphonse Renard, LL.D. Hon. M.R.S.E. Cor. G.S.
Curator of the Royal Museum, Brussels.

La Reproduction Artificielle des Roches Volcaniques.

"Neque enim aliud est natura quam ars quædam magna."
Leibniz, *Protogäa*, ix.

Prise à ses débuts, la connaissance de l'écorce terrestre est tout utilitaire, si je puis m'exprimer ainsi : dans sa première phase, elle nous apparaît comme imposée à l'homme par la nécessité d'explorer les couches du globe, pour en extraire les minerais, les matériaux de construction et les matières combustibles.

Pour quiconque jette un coup d'œil sur l'histoire des sciences, il devient évident, qu'elles doivent toutes leur origine à un but utile et pratique ; que toutes ont passé par cette phase initiale pour suivre un développement régulier, dont je vais esquisser la marche pour la géologie.

L'homme commence donc l'exploration des profondeurs terrestres pour y puiser les matières qui doivent servir à ses besoins. D'abord il le fait sans règle ; mais à mesure que l'art du mineur se développe, la recherche des richesses minérales est poursuivie avec méthode : on observe les conditions dans lesquelles les minéraux et les roches utiles se rencontrent au sein du globe. D'empiriques et de locales qu'elles étaient à l'origine, ces observations ne tardent pas à se généraliser ; elles permettent alors d'entrevoir quelques-uns des traits saillants de l'architecture de notre planète. En fouillant les entrailles de la terre, on en vint à se convaincre que le globe n'a pas été fait d'un seul coup, qu'il doit sa formation à des époques successives.

On comprit ensuite que, pour interpréter l'histoire de la terre et le rôle des causes en jeu dans son édification, il fallait se livrer à des études sur le vif : apprendre à connaître l'état actuel de notre planète. En comparant les diverses couches du globe avec les terrains qui s'édifient sous nos yeux, on parvint à retracer les conditions qui ont présidé à la formation des assises des périodes anciennes. C'est ainsi que par l'analyse des faits et par l'induction qui généralise les observations, la connaissance de l'écorce terrestre entre dans une phase nouvelle et vraiment scientifique. On s'était proposé d'abord de découvrir des règles pratiques pour le mineur et l'on est amené graduellement à déchiffrer l'histoire de la terre.

Un principe fondamental nous guide dans cette reconstitution du passé de la planète : c'est que l'essence des forces qui ont agi sur la terre, n'a jamais changé. On ne doit donc rechercher dans les époques géologiques que la trace de phénomènes dont la nature est la même, peut-on dire, que ceux dont nous sommes les témoins, et que nous pouvons soumettre à l'observation directe.

Depuis que, vers la fin du siècle dernier, on commença à appliquer la méthode inductive à l'étude des masses minérales formant l'écorce du globe, à leur architecture et aux êtres dont les débris sont enchâssés dans les terrains, un vaste ensemble de documents s'est accumulé sur l'histoire de notre planète. Durant cette période, la géologie s'est affirmée par d'immenses progrès, qui ne lui laissent rien à envier à ses aînées, les autres branches des sciences naturelles.

Voyons comment, en mettant en jeu cette méthode analytique, en s'appuyant sur l'induction, le géologue interprète la formation des roches. Les roches sont, on le sait, les masses minérales solides qui constituent les terrains. L'observation nous apprend à y distinguer un premier groupe, caractérisé par la disposition en couches ou en bancs : ce sont les roches sédimentaires. Un second groupe, qui ne nous offre point cette disposition stratifiée, comprend les roches de nature volcanique, à structure massive. La structure et la composition si différentes de ces deux grandes divisions lithologiques, nous conduit à envisager celles-ci comme formées dans des conditions spéciales, qui ont laissé leur empreinte sur chacune d'elles.

Nous voyons les roches sédimentaires s'édifier, lorsque nous observons comment les eaux courantes et la mer roulent et déposent sur leur lit des cailloux, du sable, et du limon. Après la mort des organismes qui vivent dans ces eaux, leurs squelettes ou leurs coquilles viennent se mêler aux substances minérales et élever avec celles-ci des couches de sédiments. Ces matières ainsi déposées prennent, par apport successif, la disposition stratifiée. Toutes les particules qui les constituent, étaient primitivement des grains isolés et qui portent encore la trace de leur origine : ce sont des débris de roches préexistantes ou des dépouilles d'organismes, que des actions physico-chimiques postérieures peuvent cimenter.

Comparons maintenant ces dépôts modernes sédimentaires, caractérisés par la disposition stratifiée et la nature détritique de leurs éléments, à certaines couches des formations géologiques. Nous voyons, sur les surfaces continentales, des masses dont la formation remonte aux périodes géologiques, et qui offrent des analogies étroites d'allure et de structure, avec les matériaux qui se déposent, sous nos yeux, des eaux fluviales et marines. Cette comparaison nous amène à envisager les roches stratifiées anciennes comme formées sous l'empire des mêmes causes, et nous les interprétons comme déposées sous la mer ou par les eaux courantes. C'est donc surtout l'eau qui est à l'œuvre dans l'édification des masses sédimentaires ou détritiques.

Le second groupe, dont nous aurons à traiter spécialement, com-

prend les roches massives ; celles-ci s'observent, en voie de formation, dans les manifestations volcaniques. Les matières fondues, vomies du cratère ou injectées dans les couches sédimentaires, se consolident par refroidissement. Les éléments qui constituent les laves sont des individus cristallins, développés aux dépens de la pâte en fusion qui les enchâsse. Ces cristaux ne présentent rien de détritique dans le sens que nous attachions, tout à l'heure, à ce mot. A parler d'une manière générale, la disposition par couches des terrains sédimentaires n'est pas indiquée pour ces masses éruptives : au lieu de l'horizontalité primitive, de la superposition régulière des couches stratifiées, nous avons dans les laves, une allure qui indique la poussée de bas en haut, à laquelle elles ont été soumises lors de leur éruption. Enfin, ces roches massives sont dépourvues de débris d'êtres organisés.

Comparons, à leur tour, ces produits volcaniques contemporains avec certaines roches anciennes cristallines : les granites, les porphyres, les trachytes, les basaltes. Nous observons que ces dernières possèdent, avec les produits des volcans actifs, des analogies étroites de structure et de composition. On peut conclure de cet ensemble de traits caractéristiques communs, que les roches massives, qui traversent les terrains géologiques sédimentaires, ont été, comme les laves modernes, injectées de bas en haut, et qu'elles partagent avec celles-ci une origine éruptive.

Mais tandis que nous voyons se former, sous nos yeux, les roches sédimentaires, que les conditions qui président à leur origine peuvent être suivies d'assez près, que cette œuvre s'accomplit au grand jour peut-on dire, les masses éruptives s'élaborent dans les profondeurs de la terre ; leur genèse est en quelque sorte entourée de mystère, le regard ne peut sonder les vastes réservoirs souterrains, où ces matières en fusion se pétrissent et d'où elles sont projetées lors des éruptions volcaniques.

Ici, les voies de l'observation directe sont en partie fermées ; la plus fine analyse, les raisonnements les plus serrés ne peuvent suppléer aux données qui nous manquent ; ils sont impuissants à nous apprendre toutes les causes en jeu dans la formation des roches éruptives.

Pour résoudre les doutes, contrôler et compléter les observations, on tente alors de reproduire artificiellement les roches volcaniques : d'en faire la synthèse. Armé des données de l'observation qui doivent servir de guide, on s'efforce, par des manipulations savantes, d'imiter les produits de la nature. La science de la terre, d'analytique qu'elle était, entre dès lors dans une dernière phase, celle des experiences synthétiques.

Quoique rapetissés à nos appareils, ces essais d'imiter la nature, conduits par l'intelligence de l'homme, exécutés par ses mains, lui permettent de faire naître des faits analogues à ceux qu'il veut approfondir, de diriger et de surveiller la marche des phénomènes, de se rendre un compte exact de leurs relations et de faire varier à

volonté les conditions où ils peuvent se produire. Les connaissances acquises par l'observation, l'analyse et le raisonnement sont ainsi, suivant l'expression de Bacon de Verulam, *soumises au fer et au feu de l'expérience.*

Nous venons d'indiquer à grands traits trois étapes dans la marche des connaissances relatives à l'écorce terrestre. Nous les avons entrevues à leur naissance, au moment où elles se bornaient à des notions utiles pour l'humanité; nous les avons suivies plus tard, lorsque, se guidant par l'observation et le raisonnement, elles s'élevaient à la hauteur d'une science. La géologie, entrée dans une dernière phase, se transforme aujourd'hui en science expérimentale.

Nous allons montrer en étudiant la reproduction artificielle des roches volcaniques récentes, quel puissant secours les recherches du laboratoire peuvent apporter à l'observation directe de la nature. Mais avant d'exposer les procédés de la synthèse des roches éruptives contemporaines, nous avons à résumer ce que l'analyse et l'observation des faits ont appris sur la constitution et le mode de formation de ces masses volcaniques. Le point de comparaison des synthèses se trouve dans les laves naturelles; ce sont des modèles qu'on veut copier: il importe donc de les connaître par le menu pour arriver à les imiter dans les détails intimes.

Rappelons donc ce que nous savons des laves et des conditions qui président à leur formation. Nous n'avons pas à nous arrêter ici sur les grandes manifestations des forces internes du globe, ni sur le cortège de phénomènes qui les accompagnent, sur ces éruptions formidables qui ébranlent le volcan jusque dans ses fondements, et projettent des masses vitreuses pulvérisées et des pierres embrasées. Au milieu de cet cataclysme, le cratère et les flancs de la montagne, entrouverts sous la poussée des masses éruptives, laissent échapper des flots de laves, qui se déroulent sur les pentes, et s'y solidifient lentement.

Le fait capital de l'éruption c'est l'émission de la lave. On appelle ainsi les courants de matières fondues qui s'échappent du cratere. A parler d'une manière générale, on ne peut mieux comparer la lave qu'à un verre liquéfié sous l'influence des hautes températures qui règnent sous l'écorce solide du globe. Les observations directes sur la température de la lave liquifiée du cratère, faites au moment même de l'éruption, sont entourées de périls que peu d'observateurs ont osé affronter. Aussi ne possédons-nous, sur ce point, que des indications approximatives. Les volcans, où l'épanchement des laves ne dépasse pas un certain degré d'intensité, mais qui sont dans un état d'activité modérée et permanente, comme à l'île Hawaï, ont permis à d'intrépides savants d'approcher assez près du cratère pour tenter d'évaluer la température de la masse liquifiée. Ils ont pu constater ainsi qu'elle oscille entre 1000° et 2000° C. Mais dès que la lave s'épanche, la température baisse rapidement à sa surface; la nappe liquide se recouvre d'une couche, plus ou moins épaisse, de

scories sous laquelle coule comme un ruisseau de matières fondues, qui atteignent encore le point de fusion de l'acier. C'est ce manteau de scories qui empêche l'irradiation et qui permet aux masses, qu'il recouvre, de conserver longtemps une certaine viscosité.

Nous indiquerons plus loin les observations sur les phénomènes de cristallisation qui se passent dans ces matières épanchées, encore fluides ou visqueuses, prêtes à se figer. Voyons tout d'abord quelques-uns des caractères essentiels de la structure et de la composition des laves. Souvent ces produits éruptifs sont bulleux, scoriacés; quelquefois, au contraire, ils se présentent comme une masse vitreuse, homogène, de teinte plus ou moins foncée, où l'œil nu ne distingue aucun minéral isolé. Quelquefois aussi cette masse est comme pétrie de minéraux, dont la présence, en nombre plus ou moins considérable, semble refouler la pâte vitreuse qui les cimente. Ces minéraux ainsi empâtés présentent, lorsque leur développement est complet, des formes polyédriques régulières constantes pour chacune des espèces: se sont des cristaux, c'est à dire, les individus parfaits du monde minéral. Ils ont puisé dans la masse primitive vitreuse, les éléments chimiques qui les constituent et qui se sont groupés suivant leurs affinités; comme on voit s'engendrer, dans un liquide saturé d'un sel, les cristaux dont la substance était dissoute dans l'eau mère.

La minéralogie nous apprend à déterminer les espèces minérales qui cristallisent dans les laves. L'analyse chimique vient à son tour nous fournir de précieuses indications sur la composition des produits volcaniques. Si l'on traite, par les procédés de la chimie, les roches éruptives, on trouve que toutes contiennent une quantité plus ou moins considérable de silice combinée, qui peut s'élever au delà de 65 pour cent de la masse: ce sont les laves acides ou légères. On passe ensuite par toutes les transitions aux laves basiques ou lourdes, dont la teneur en silice, diminuant graduellement, n'atteint plus que 55 à 45 pour cent. La silice dont il s'agit n'existe pas à l'état libre dans les laves contemporaines; cette substance y est combinée, sous la forme de silicates, avec l'alumine, le fer, la chaux, la magnésie, la potasse et la soude.

Nous trouvons dans les laitiers de l'industrie métallurgique des produits présentant des analogies intimes avec ceux des volcans, tant au point de vue de la composition que du mode de formation. Ces scories artificielles sont, ainsi que les laves, formées de silicates et ce qui rapproche encore ces dernières des laitiers, c'est qu'elles doivent être envisagées comme l'écume du noyau métallique interne, dont elles formeraient les zones supérieures. Les différences que nous montre leur composition résulteraient du fait qu'elles nous viennent de zones plus ou moins profondes.

Nos connaissances sur les roches éruptives devaient s'enrichir d'une manière inespérée par l'application du microscope à la lithologie. Nous n'avons pas à rappeler ici les résultats presque merveilleux qu'a permis d'atteindre ce mode d'investigation, inauguré par H. C. Sorby. Pour tout dire en un mot, l'analyse microscopique

des roches a changé la face de la pétrographie. Envisageons seulement quelques-unes des notions sur les produits volcaniques récents, révélées par ces nouveaux procédés dont la délicatesse, la sureté, et l'élégance n'ont été surpassées dans aucune autre branche des sciences naturelles. Non seulement ils ont rendu possible la vérification et le contrôle des hypothèses; mais ils les ont guidées et fait aboutir aux remarquables découvertes que je vais rappeler.

L'œil aidé des plus fortes loupes ne pouvait reconnaître dans les laves, que les minéraux cristallisés d'assez grandes dimensions; l'analyse chimique ne donnait, le plus souvent, que la composition de la masse totale; la constitution minéralogique n'était qu'entrevue. La texture intime de la roche restait impénétrable ; on ne pouvait se rendre compte d'une manière certaine de l'ordre suivant lequel les éléments de cette masse fondue s'étaient solidifiés, ni se représenter les divers états par où passent les cristaux, leurs ébauches, leurs formes primordiales, leurs squelettes, et l'aspect de la roche à ses différents stades de développement. Appliquons le microscope à l'examen d'un mince éclat de lave, rendu transparent par le polissage. Les laves, avons-nous dit, peuvent être comparées à des masses vitreuses; mais tandis que dans nos verres artificiels on s'efforce d'obtenir un produit homogène et limpide, les matières liquifiées des volcans, quand elles arrivent au jour, apportent déjà dans leur masse, des éléments différenciés. Le verre qui les renferme doit être considéré comme un résidu de cristallisation où de nombreux individus cristallins ont puisé les éléments qui constituent leur espèce. Dans ces verres volcaniques noirs, brillants, opaques en apparence, dépourvus de toute cristallinité, le microscope découvre un monde de formes minérales. Il nous montre leurs divers états de croissance, leurs arrêts de développement déterminés par la consolidation plus ou moins rapide de la masse. C'est surtout dans les roches qui ont conservé à peu près totalement leur nature vitreuse, homogènes à l'œil nu comme l'obsidienne, qu'on trouve ces cristaux rudimentaires de figure bizarre, premiers pas de la matière amorphe dans son passage à l'état cristallin. Grâce à la rapidité avec laquelle la pâte vitreuse s'est figée, les cristaux n'ont pu croître ; leur développement a été brusquement arrêté. De là ces embryons de cristaux qui abondent dans les verres naturels, et qu'on a désignés sous le nom de *cristallites.* Des cristallites analogues se produisent dans les laitiers des hauts-fourneaux, dont nous indiquions tout à l'heure les relations étroites qui les unissent aux matières laviques. Cette commune origine se traduit par des traits de famille qu'accuse le microscope. Les laitiers examinés en lames minces montrent des formes cristallines rudimentaires semblables aux cristallites des verres volcaniques.

Mais d'ordinaire les cristaux ne sont pas restés à cet état embryonnaire. Si la lave ne s'est pas trop brusquement refroidie, les mouvements moléculaires, conservant leur liberté d'action, même dans une masse semi-liquide, la pâte a pu donner naissance à des individus cristallisés de très petite dimension, les *microlithes.* Ces cristaux

microscopiques sont nés au sein de la masse vitreuse pendant qu'elle se consolidait lentement. Malgré leur infinie délicatesse, ces petits polyèdres permettent de retrouver, avec une exactitude merveilleuse, tous les caractères propres à des espèces, qu'on ne connaissait dans le monde minéral qu'en individus beaucoup plus volumineux, et dont, à coup sûr, on ne pouvait soupçonner la présence dans les laves. Ils forment souvent dans la pâte, par leur enchevêtrement, un admirable réseau cristallin et prêtent à la roche, où ils se sont developpés, une structure spéciale, la *structure microlithique.*

Les dimensions toujours microscopiques de ces microlithes, leur agencement, montrent bien qu'ils appartiennent à une période troublée, qu'ils ont été formés à un moment où la lave, encore en mouvement, se solidifiait; ils s'en sont séparés pendant l'acte même de l'épanchement ou de l'éruption.

Outre ces cristaux microscopiques et ces groupements de cristallites, qui sont du dernier stade de consolidation, la lave apporte avec elle une provision de cristaux plus grands et de forme plus developpée qu'on peut bien souvent distinguer à l'œil nu. Ces derniers ont pris naissance dans des conditions plus calmes, analogues à celles que présente un fluide tranquille où la cristallisation a pu se faire d'une manière lente. Ils se sont formés dans le bain chimique en fusion, alors qu'il était encore renfermé dans les réservoirs souterrains. Cet accroissement lent nous est montré d'une manière évidente par leur disposition en zones concentriques et par leurs dimensions. Ces grands cristaux, apportés tout formés dans la lave, au moment de son éruption, sont enchâssés dans des microlithes ou dans une masse vitreuse. C'est après qu'ils s'étaient développés avec lenteur dans le magma, durant la phase intratellurique, que la masse où ils nageaient a été soulevée. Une période d'agitation a succédé au calme, la lave entraînée avec violence a charrié ces cristaux, les a brisés, corrodés, broyés, et fondus en partie. Le microscope nous montre nettement les phénomènes que je rappelle. On voit les grands cristaux disloqués, leurs fragments sont dispersés, leurs arêtes émoussées, et rongées; ils sont envahis et pénétrés par la pâte.

Pendant que les actions physiques et chimiques, mises en jeu par le mouvement de la lave, s'attaquent ainsi à démolir les cristaux anciens, naissent les microlithes. Cette matière vitreuse, où flottent les grands cristaux, se prend en un amas d'individus microscopiques. Ceux-ci se rattachent donc à une seconde phase de la cristallisation, ils sont engendrés dans un magma visqueux en mouvement; leur développement ultérieur est arrêté par un refroidissement assez rapide qui provoque la prise en masse.

La disposition fluidale des microlithes indique parfaitement d'ailleurs que cette poussée cristalline a été contemporaine du mouvement de la coulée. On remarque, en effet, dans les préparations microscopiques que les microlithes s'accumulent autour des grandes sections cristallisées; ils ondulent, forment des traînées et présentent cette disposition que les micrographes appellent *structure fluidale.*

Elle est accusée par l'orientation de ces cristaux aciculaires infiniment petits. Lorsque ces traînées de microlithes viennent à rencontrer des cristaux enclavés de dimensions plus considérables, ils les contournent, se pressent dans les interstices qui séparent les grandes sections, s'appliquent sur leurs bords et nous offrent le dernier mouvement de la masse au moment même où elle se figea.

Le microscope nous enseigne donc que la cristallisation dans les laves appartient à deux temps. Le premier antérieur à l'éruption, durant lequel les grands cristaux déjà formés nagent dans une masse qu'on peut supposer entièrement vitreuse. Au second temps, les microlithes et les formes cristallines embryonnaires s'isolent; ils datent de l'éjaculation, de l'épanchement même, et sont contemporains de la consolidation de la roche.

Ces observations microscopiques sur les cristaux de la seconde phase permettent déjà de conclure qu'ils ont été formés purement et simplement par voie ignée, sans qu'on doive faire entrer en jeu des températures ou des pressions hypothétiques, auxquelles on recourait autrefois; sans réclamer un repos absolu qu'on envisageait comme nécessaire pour que des minéraux puissent cristalliser régulièrement. On voit, en effet, les microlithes se former après l'épanchement, à la pression barométrique, à une température qui est loin d'être aussi élevée qu'on la supposait; on voit les cristaux naître pendant la marche même de la coulée. Lorsque le refroidissement est très brusque, les microlithes n'ont pas le temps de se former, la matière lavique ne donne naissance alors qu'à des cristallites.

Mais le microscope nous permet de fixer d'une manière plus détaillée encore la chronologie des cristaux des laves; nous venons de distinguer dans leur histoire, deux grandes périodes; indiquons d'une manière générale, comment on peut, en quelque sorte, établir la date à laquelle chacune des espèces de ces deux groupes se sont isolées du verre. Les particularités qui conduisent à la détermination de leur âge relatif, ce sont les inclusions.

Un cristal qui se développe d'une masse vitreuse, englobe souvent des particules du milieu dans lequel il croît. C'est ainsi que certaines sections apparaissent au microscope criblées de grains vitreux, emprisonnés à l'intérieur du cristal et souvent disposés suivant les zones d'accroissement. Ces inclusions nous montrent à l'évidence que les cristaux en question sont nés d'une matière vitreuse liquifiée par la chaleur. Dans d'autres cas, ce sont des espèces minérales qui se trouvent incluses sous la forme de microlithes, au sein d'un cristal. Il est évident alors que ces microlithes préexistaient au minéral qui les emprisonne.

Dans d'autres cas enfin, sur des cristaux nettement terminés, une espèce vient se mouler, s'appliquer, remplir tous les interstices entre les minéraux déjà formés: ceci montre incontestablement l'antériorité de ces derniers.

En tenant compte de ces faits, qui parlent par eux-mêmes, on est parvenu à dresser des listes chronologiques indiquant pour chacune

des espèces des deux grandes périodes, la date de la cristallisation. Je ne m'arrêterai pas à vous les citer, mais nous verrons bientôt se dégager par les expériences synthétiques, la loi qui préside à la formation successive des cristàux et à leur âge relatif.

J'ai retracé les grandes lignes du tableau qui nous offre l'histoire d'une lave ; je n'ai pu esquisser que certains détails de cette représentation des phénomènes lithologiques que les investigations modernes ont rendus avec une si vivante réalité ; mais ce que nous en avons vu, suffit à montrer d'une manière frappante, à mon avis, ce que peut l'analyse secondée par le raisonnement. Je crois ne pas me tromper en avançant, qu'à ce point de vue, l'étude d'une lave, telle que nous nous sommes efforcés d'en exposer les résultats, présente un des plus beaux exemples de l'application des méthodes inductives aux sciences naturelles : on ne sait ce que l'on doit le plus y admirer, ou des procédés mis en œuvre pour l'analyse, ou de la finesse de l'observation, ou du lien logique avec lequel on a su rattacher tous ces phénomènes.

Pouvoir retracer avec une stricte fidélité dans une masse rocheuse, où l'œil nu ne découvre qu'un amas indistinct et tout d'une venue, la marche de la cristallisation, pénétrer dans cet admirable tissu des produits volcaniques où, dans un centimètre cube, viennent s'agencer des millions de polyèdres, déterminer avec une précision mathématique la nature de chacun de ces corps infiniment petits, les prendre à leur naissance, les suivre jusqu'à leur entier développement, retrouver la trace de toutes les modifications qu'ils ont pu subir sous l'influence des agents physiques et chimiques, voilà ce que ce puissant mode d'investigation, l'analyse microscopique, a permis de réaliser.

Toutefois pour le chercheur consciencieux et modeste, que de choses encore inconnues dans ce champ en apparence si restreint et déjà si bien fouillé de l'histoire des produits volcaniques ! Que de problèmes dont la solution ne peut être donnée même par l'observation la mieux conduite ! Lorsque l'observation ne suffit plus à atteindre ce but, lorsqu'on a épuisé toutes les ressources de ce mode d'investigation, il reste encore celles des expériences synthétiques. C'est un pas de plus dans la voie qui mène à l'intelligence complète des faits et qui peut conduire à des solutions définitives. Mais, les opérations synthétiques pour arriver à ce but, doivent être dirigées avec intelligence et dessein vers la fin qu'on veut atteindre.

Comme l'a dit Sénarmont, l'une des conditions essentielles d'une synthèse géologique, c'est que chacune des opérations artificielles, soit compatible avec toutes les circonstances où l'opération naturelle a laissé des traces caractéristiques. Les laitiers et les scories de l'industrie, dont nous avons montré, les relations avec certains produits de la nature, sont en réalité des synthèses, mais des synthèses de hazard, qui, malgré le haut intérêt scientifique qu'elles présentent, ne peuvent être mises sur le même pied que les synthèses intentionnelles, dont je vais parler, où l'expérimentateur, tenant en vue le problème à résoudre, s'efforce de réaliser, dans le laboratoire, des conditions

identiques à celles qui entouraient la formation des produits naturels qu'on veut imiter.

Dans l'ordre logique, les méthodes synthétiques suivent en quelque sorte les progrès de l'observation et de l'analyse. Cependant on constate que, dès les premiers pas de la géologie, quelques hommes supérieurs entrevoient déjà, avec le coup d'œil du génie, le rôle que l'expérience est appelée à jouer dans cette science. Buffon démontre par des essais que le granite et les principales roches cristallines sont fusibles et qu'elles se transforment par la fusion en matière vitreuse. Quelques années après, Spallanzani exécute une longue série d'expériences sur la fusion de laves, pour détruire les préjugés qui régnaient sur la cause de la chaleur des matières éruptives.

Mais c'est surtout à Sir James Hall que revient l'honneur d'avoir, par des essais restés célèbres, inauguré l'expérience en géologie ; il en a démontré l'application d'une manière magistrale et il l'a généralisée. Nous n'avons à envisager dans les travaux de Hall, que ceux qui touchent à la synthèse des roches. Vers l'époque où Spallanzani étudiait, par les procédés du laboratoire, les conditions de formation des laves, l'illustre géologue écossais fondait des roches éruptives dans un récipient en graphite ; il observait que le produit de cette fusion, refroidi brusquement, donnait une masse vitreuse amorphe, tandis qu'un refroidissement plus lent y provoquait la formation de cristaux. James Hall avait déjà reconnu par l'expérience, ce fait capital pour les synthèses futures, que, pour régénérer les cristaux d'une roche qu'on a fondue, il faut maintenir le verre provenant de la fusion, à une température élevée ; mais inférieure toutefois à la chaleur à laquelle on a dû recourir pour fondre la roche. Durant ce recuit, certains minéraux peuvent cristalliser. Ces faits sont à mettre en parallèle avec ceux que nous montrent les laves au moment où la température s'abaisse après l'épanchement.

Vers le commencement de ce siècle, Gregory Watt dirige ses recherches dans la même voie : il expérimente sur des masses de basalte de 700 livres, il les fond et les laisse refroidir pendant huit jours sous une couche de charbon qui se consumait lentement. Durant ce recuit prolongé, des concrétions sphérolithiques fibro-radiées, de six centimètres de diamètre, s'isolaient dans le verre noir et opaque obtenu par la fusion du basalte ; enfin ce verre passait à l'état pierreux, devenait grenu, se chargeait de lamelles cristallines très minces. En même temps, son magnétisme augmentait et sa densité croissait de 2,743 à 2,949.

Une conclusion des recherches de Watt, qui se rattache par bien des points à celle de Hall que nous venons d'exprimer, c'est que la cristallisation peut se produire dans une période où la matière fondue commence à se solidifier.

Au moment où l'on préparait ainsi les voies de la synthèse des roches, l'analyse et les moyens d'investigation n'avaient pas atteint la

perfection qu'elles possèdent aujourd'hui ; d'un autre côté, les préjugés qui régnaient aux débuts de la géologie accumulaient des obstacles qu'on ne devait surmonter qu'un demi-siècle plus tard. Nous n'avons pas à nous arrêter ici sur la brillante période des synthèses minérales qui suivit de près l'essor de la chimie et de la minéralogie. Il suffit de citer les noms d'Ebelmen, de Rose, de Mitscherlich, de Sénarmont, pour évoquer le souvenir des remarquables résultats de la reproduction artificielle des minéraux. Mais les recherches de ces savants, portaient principalement sur la synthèse d'espèces isolées et non sur les roches, qui sont des agrégats d'espèces minérales. À parler d'une manière générale, leurs expériences étaient surtout d'ordre minéralogique, et ne touchaient que secondairement à la lithologie. Toutefois les essais de ces habiles expérimentateurs, éclairèrent bien des problèmes géologiques. Ils nous prouvent aussi comment s'est maintenue et accentuée, à mesure que se développaient les sciences minérales, cette tendance qui porte l'intelligence à chercher, par les méthodes expérimentales, la compréhension plus complète des phénomènes de la nature. Enfin en 1866, Daubrée trace la voie de la reproduction des roches cristallines par fusion simple. C'est sa méthode qui fut reprise depuis, et développée par MM. Fouqué et Michel Lévy. Les recherches de Daubrée auxquelles il est fait allusion ici, sont celles qu'il entreprit pour reproduire par la fusion certaines pierres météoriques caractérisées par l'absence de l'élément feldspathique. Il fondait une roche terrestre, la lherzolite, dont la composition se rapproche des météorites correspondantes, et parvenait à obtenir des produits qui dans les détails de la structure et de la composition copiaient ceux des types cosmiques qu'il voulait imiter.

Au moment où cet éminent géologue préludait ainsi aux recherches qui devaient, quelques années après, jeter un si vif éclat sur le laboratoire de géologie du Collège de France, la voie des méthodes synthétiques était encore encombrée par les hypothèses. Ce n'était plus avec celles relatives à l'influence de forces mystérieuses qu'on avait à lutter ; mais on pensait que la reproduction des phénomènes géologiques dans le laboratoire n'était possible qu'à la condition de disposer de temps d'une durée infinie, de températures et de masses dont celles que nous pouvions mettre en jeu ne donnaient pas même l'idée. On supposait encore que les associations minérales de la nature se réglaient suivant d'autres lois que celles des combinaisons que produisait le chimiste. Évidemment ce ne sont pas ces préjugés qui ont arrêté Daubrée dans la route où, par la synthèse des météorites, il avait si vaillamment fait le premier pas. Il est un de ceux, hâtons-nous de la dire, dont les travaux ont le plus contribué à faire disparaître ces hypothèses du domaine de la géologie. Mais les méthodes d'analyse telles qu'elles existaient alors ne permettaient pas encore de pénétrer à fond la nature des roches naturelles et de comparer leur structure intime avec celle des produits de la synthèse. Les laboratoires ne possédaient pas les appareils au moyen desquels

on peut obtenir de très hautes températures en les maintenant fixes pendant le temps prolongé que réclament les expériences.

Les grands progrès réalisés dans la construction de ces appareils et l'application du microscope à la lithologie, vinrent enfin permettre d'aborder la reproduction de toutes les roches volcaniques contemporaines. Deux savants français, MM. Fouqué et Michel Lévy, qui avaient introduit dans leur pays la lithologie micrographique, commencent, en 1877, une série d'expériences synthétiques désormais mémorables dans l'histoire de la science. L'un d'eux, s'était acquis une juste réputation, par ses remarquables travaux sur les phénomènes des volcans, qu'il avait suivis sur place dans les diverses régions classiques; il était familiarisé avec tous les secrets de l'analyse chimique minérale, qu'il a dotée des méthodes les plus ingénieuses et les plus utiles. L'autre, préparé par les fortes études des hautes écoles françaises, avait abordé avec un éclatant succès l'examen des minéraux par leurs propriétés optiques; il avait porté plus avant qu'on ne l'avait fait avant lui, l'application des méthodes exactes en micrographie, et s'était fait connaître par ses recherches sur les roches éruptives de la série ancienne.

Dans leurs travaux faits en commun, MM. Fouqué et Lévy avaient en quelque sorte systématisé et coordonné les faits relatifs à la succession chronologique des cristaux des roches éruptives et révélé un grand nombre des détails que nous avons signalés en exposant les résultats de l'analyse des laves. C'est à cette heureuse association de talents, à cette féconde collaboration, que l'on doit les belles découvertes qui ont rendu célèbre le laboratoire du Collège de France et auxquelles c'est un honneur pour moi de pouvoir rendre hommage devant un auditoire chez qui tous les progrès scientifiques sont accueillis avec faveur, et de cette tribune, la première au monde pour la diffusion des sciences, où l'immortel Faraday exposait jadis, avec une ardeur généreuse, les admirables travaux de synthèse minéralogique d'Ebelmen.

Nous avons indiqué déjà les données sur lesquelles ces savants devaient s'appuyer dans leurs essais: ce sont celles fournies par l'analyse chimique et minéralogique. Un point, que nous n'avons pas encore touché, est la base de leur procédé général. Comme la théorie pouvait le prévoir, les cristaux les plus anciens d'une roche ignée doivent être les moins fusibles. A parler d'une manière générale, c'est d'ailleurs ce qu'on observe: les minéraux du premier temps de la cristallisation sont ceux qui occupent le degré le plus bas de l'échelle de fusibilité. Les espèces constitutives des laves ont apparu à des temps successifs, suivant leur degré de fusibilité, à mesure que la température décroissait. Ces faits constatés en détail par l'analyse mcroscopique ont servi de point de départ aux manipulations de MM. Fouqué et Lévy. Leur procédé repose d'autre part sur un fait que James Hall avait entrevu: c'est que la fusion d'une roche produit un verre plus facilement fusible, que ne le sont chacune des espèces cristallines constitutives de cette roche. Or, si l'on fond un agrégat

naturel de minéraux et qu'on fasse passer le verre, produit de cette fusion, par une série de températures décroissantes, mais toujours supérieures à celle de la fusion de cette masse vitreuse, les minéraux qui peuvent cristalliser de ce magma doivent naître les uns après les autres et les moins fusibles seront les premiers à s'isoler. Ces cristaux seront englobés, moulés par ceux dont la fusibilité est plus grande, et qui vont apparaître, à leur tour, à mesure qu'on fera décroître la température. Sans insister sur la description technique des appareils, bornons-nous à dire, qu'à l'aide des fourneaux et des trompes, dont se servent pour leurs synthèses MM. Fouqué et Lévy, on obtient tous les degrés intermédiaires entre le rouge sombre et le blanc éblouissant et qu'on peut maintenir constante une température donnée pendant un temps illimité.

On introduit dans le fourneau un creuset en platine, d'une capacité d'environ 20 centimètres cubes, renfermant le mélange de matières minérales que la fusion et les recuits vont transformer en roche. Voici les phases des opérations : d'abord, à l'aide de dispositifs spéciaux, on porte pendant quelque temps la température au blanc éblouissant, le mélange se transforme en verre. En réglant l'admission du gaz et de l'air, en découvrant le fourneau on fait décroître la température de la masse fondue jusqu'au rouge orangé, point de fusion de l'acier. On soulève ensuite le creuset hors du fourneau, où la température décroît au rouge cerise, point de fusion du cuivre. Enfin si l'on fait sortir complètement le creuset du four, on peut encore le maintenir à une température où le cuivre fondrait mais difficilement.

Nous avons indiqué les grandes lignes de la marche de l'opération. Ce sont ces recuits successifs à des températures décroissantes, qui forcent les cristaux à se former en série à commencer par les moins fusibles et qui permettent de donner aux matières fondues, soumises à ces manipulations, la texture et la composition minéralogique des produits volcaniques.

Nous allons montrer par quelques exemples, le mode opératoire des synthèses lithologiques. Suivons d'abord les manipulations pour la reproduction d'une des roches qui jouent le rôle principal dans les éruptions du Vésuve : la leucotéphrite. Cette roche est composée de leucite, de labrador et d'augite.

On forme un mélange de silice, d'alumine, de chaux, d'oxyde de fer, de potasse et de soude qui répond à 1 partie d'augite, 4 de labrador, 8 de leucite. On introduit ce mélange dans le creuset et on le transforme, au blanc éblouissant, en un verre homogène. Dès que la fusion des éléments chimiques est opérée, on abaisse la température, et durant 48 heures, on maintient la matière vitreuse à la température de l'acier fondu. Les cristaux de leucite s'isolent durant cette première phase de l'opération. Elle répond évidemment au premier temps de la consolidation des roches éruptives.

On maintient de nouveau pendant 48 heures la matière à la température de fusion du cuivre, toute la masse, le résidu d'où s'étaient séparés, dans le premier temps, les cristaux de leucite, se transforme

en microlithes d'augite, de labrador, en octaèdres de magnétite et de picotite.

Comparons maintenant, après ce double recuit, les préparations microscopiques de synthèse de la lave naturelle ; non seulement les mêmes minéraux ont été reproduits par le procédé exclusif de la fusion sèche, mais l'ordre de leur apparition, la proportion des espèces constitutives est identique et cette analogie peut se poursuivre même dans les détails des formes cristallographiques. La leucite en grands cristaux offre toutes les particularités de ce minéral dans les laves vésuviennes, autour d'eux viennent se grouper les microlithes du deuxième temps—l'augite et le labrador. Enfin, comme dans la roche naturelle, la leucite contient des inclusions de fer magnétique et de picotite qui sont les minéraux les plus anciens.

Prenons comme second exemple, la synthèse du basalte, l'un des types les plus répandus de la série volcanique et au sujet duquel bien des hypothèses avaient été avancées pour en expliquer l'origine. On sait que le basalte est composé essentiellement de trois minéraux : l'olivine, l'augite et le labrador. L'olivine dans la roche naturelle apparaît en cristaux de la première consolidation.

Comme dans le cas de la leucotéphrite on forme un mélange d'éléments chimiques ou de minéraux pulvérisés répondant à la composition moyenne d'un basalte riche en olivine. Ce mélange est composé de 3 parties de ce minéral, 2 d'augite et 3 de labrador. On le transforme d'abord en un verre homogène noir. Pendant 48 heures on le maintient au rouge blanc. Si, après ce recuit à haute température, on examine une lame mince de ce verre on y observe de grands cristaux d'olivine. Ceux-ci sont encore empâtés dans une masse vitreuse où de petits octaèdres de massicotite et de picotite se sont isolés ainsi que de rares cristaux d'augite.

Il reste maintenant à faire naître les microlithes de la seconde consolidation entre lesquels doivent s'enchâsser les cristaux d'olivine que nous venons de voir se développer durant la première phase. A cet effet, on maintient pendant 48 heures le culot à la température du rouge cerise. Après le recuit on a obtenu la formation d'une pâte composée de microlithes de labrador et d'augite, de magnétite et de substance vitreuse résidu de la cristallisation. Dans cette seconde phase on a donc reproduit la structure microlithique. Ces manipulations donnent naissance à des basaltes qu'on peut à peine distinguer des roches naturelles et ces quelques grammes de substance habilement maniés, nous fournissent la preuve la plus convainquante de la formation purement ignée de cette roche.

Nous pourrions exposer, ici, la remarquable série d'essais, exécutés par MM. Fouqué et Lévy, où nous avons pris les deux synthèses qui précèdent. Toutes les roches éruptives contemporaines ont été reconstituées ainsi : les andésites, les labradorites, les basaltes, les limburgites, les néphelinites, les téphrites, les roches à leucite, les péridodites, les labradorites à structure ophitique. Bornons-nous à montrer par un dernier exemple, comment ces procédés de synthèse

parviennent à éclairer, directement, les phénomènes éruptifs des périodes du passé du globe.

On avait distingué des roches cristallines anciennes, fréquentes dans les Pyrénées, sous le nom d'ophites. La période à laquelle remonte leur apparition et leur origine n'était pas établies avec certitude, lorsqu'en 1877, M. Lévy, fit voir qu'elles étaient éruptives et qu'elles montraient au microscope une structure remarquable qu'il désigna sous le nom de structure ophitique: le feldspath y apparaît englobé par des plages très grandes d'augite. Il semblait donc que ces roches ophitiques soient des roches ignées, dans lesquelles le refroidissement aurait été plus lent que dans les roches ordinaires des éruptions contemporaines. Il fallait, en tentant de reproduire le type ophitique par la synthèse, faire cristalliser l'augite durant une phase nettement séparée de celle où se produirait le feldspath, et donner, en outre, à la première, le temps de cristalliser en larges plages. A cet effet, un mélange d'une partie d'anorthite et d'augite fut soumis, après fusion, à un premier recuit auquel on le maintint pendant quatre jours, à la température de la fusion de l'acier; l'anorthite s'isole. Un second recuit de même durée à la température de fusion du cuivre, amène la cristallisation de l'augite en grandes plages, qui moulent l'élément feldspathique, et auquel viennent s'ajouter de petits octaèdres de magnétite et de picotite. L'origine éruptive des ophites et la cause de leur structure étaient donc établies, d'une manière incontestable, par cette remarquable synthèse.

On voit ressortir à l'évidence comment la synthèse parvient à éclairer la genèse des roches, à trancher les discussions qui, jusqu'à ces derniers temps, s'élevaient encore au sujet des principaux types cristallins de l'époque moderne: celles relatives aux basaltes, par exemple, où l'on voulait voir l'eau jouer un rôle important. Or, la conclusion générale qui s'impose, après ces expériences, c'est que le basalte et en général toutes les roches volcaniques des éruptions contemporaines sont de fusion purement ignée.

Mais à côté de ces magnifiques résultats, ces savants ont eu à enregistrer bien des tentatives infructueuses. Il est utile de les rappeler pour l'exemple, pour montrer les voies qu'il faut éviter, si l'on veut arriver au but. Ces insuccès circonscrivent le champ des expériences futures et tracent les limites entre lesquelles devront se mouvoir les hypothèsés. Ils démontrent, en outre, que les roches, dont on n'a pas réalisé la synthèse, par les méthodes mises en jeu, ont été formées dans des conditions différentes de celles où se constituent les produits volcaniques actuels. Cette conclusion, à laquelle l'observation et l'analyse avaient déjà conduit, sans toutefois rien préciser quant aux causes, se trouve donc confirmée par l'insuccès de la synthèse. Si elle a réussi, à refaire de toutes pièces les laves des volcans modernes, elle a échoué à imiter celles qui ont cessé de se produire dans les éruptions contemporaines. On peut dire, d'une manière générale, que, jusqu'ici, toutes les roches acides se sont dérobés aux expériences synthétiques, comme toutes celles qui ren-

ferment parmi leurs minéraux constitutifs, du quartz, du mica, de l'orthose et de la hornblende.

Les procédés de la nature n'offrent point de forces occultes ; peut-être qu'en combinant celles dont nous disposons déjà, en les modifiant dans leur application, nous sera-t-il permis de voir réaliser la production de roches qui, jusqu'aujourd'hui, se sont dérobées aux efforts. Cet espoir est établi sur les résultats atteints, qui peuvent servir de présages à de plus surprenants encore. C'est le cas de répéter que les échecs du passé préparent les conquêtes du lendemain.

Je me suis efforcé dans cette rapide revue des progrès de la synthèse lithologique, de montrer la haute portée scientifique des recherches instituées au laboratoire de géologie du Collège de France ; j'aurais pu énumérer encore les synthèses, non moins remarquables des minéraux et des météorites que les savants auteurs ou leurs élèves, parmi lesquels M. Bourgeois occupe une place à part, ont su mener à bonne fin. Mais, je dois me limiter, et ce que j'ai dit suffit à prouver combien leurs méthodes ont fait avancer nos connaissances dans un domaine de la nature dont l'accès paraissait fermé aux investigations.

Partout où, jusqu'ici, la méthode expérimentale a porté son flambeau, elle a mis en pleine lumière, les phénomènes les plus saillants de la science de la terre : il suffit de citer le nom de Daubrée, le descendant direct de ces illustres géologues de l'école écossaise, pour indiquer l'étendue du champ des sciences minérales, déjà exploré par les procédés de l'expérience. Tour à tour, ils ont été appliqués avec succès à l'interprétation des dépôts métallifères et des roches métamorphiques, aux phénomènes de trituration et de transport des matières sédimentaires, à l'étude des cassures et des déformations de l'écorce terrestre, de la schistosité des roches, de certains traits de la structure des montagnes.

La géologie, après avoir passé par les phases successives de l'observation et de l'analyse, est donc entrée dans celle de l'expérience et de la synthèse, où l'on s'efforce d'imiter la puissance créatrice de la nature, couronnant ainsi l'édifice scientifique par des procédés qui permettent d'entrevoir l'action des causes dont la connaissance est le but final des sciénces physiques et naturelles. C'est ce couronnement de l'œuvre que pressentait déjà Leibniz, lorsqu'il écrivait, il y a deux siècles :—"Il fera, selon nous, une œuvre importante celui qui comparera, soigneusement, les produits tirés du sein de la terre avec ceux des laboratoires ; car alors brilleront, à nos yeux, les rapports frappants qui existent entre les produits de la nature et ceux de l'art. Bien que le Créateur inépuisable des choses ait en son pouvoir des moyens divers d'effectuer ce qu'il veut, il se plaît, néanmoins, dans la constance au milieu de la variété de ses œuvres ; et c'est déjà un grand pas vers la connaissance des choses que d'avoir trouvé, seulement, un moyen de les produire : car la nature n'est qu'un art en plus grand."

[A. R.]

WEEKLY EVENING MEETING,

Friday, May 25, 1888.

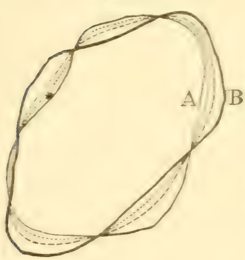
JOHN RAE, M.D. LL.D. F.R.S. Vice-President, in the Chair.

FRANCIS GALTON, Esq. M.A. F.R.S. *M.R.I.*

*Personal Identification and Description.**

IT is strange that we should not have acquired more power of describing form and personal features than we actually possess. For my own part I have frequently chafed under the sense of inability to verbally explain hereditary resemblances and types of features, and to describe irregular outlines of many different kinds, which I will not now particularise. At last I tried to relieve myself as far as might be from this embarrassment, and took considerable trouble, and made many experiments. The net result is that while there appear to be many ways of approximately effecting what is wanted, it is difficult as yet to select the best of them with enough assurance to justify a plunge into a rather serious undertaking. According to the French proverb, the better has thus far proved an enemy to the passably good, so I cannot go much into detail at present, but will chiefly dwell on general principles.

FIG. 1.



Measure of Resemblance.—We recognise different degrees of likeness and unlikeness, though I am not aware that attempts have as yet been made to measure them. This can be done if we take for our unit the *least discernible difference.* The application of this principle to irregular contours is particularly easy. Fig. 1 shows two such contours, A and B, which might be meteorological, geographical, or anything else. They are drawn with firm lines, but of different strengths for the sake of distinction. They contain the same area, and are so superimposed as to lie as fairly one over the other as may be. Now draw a broken contour which we will call C, equally subdividing the intervals between A and B; then C will be more like A than B was. Again draw a dotted contour, D, equally subdividing the intervals between C and A; the likeness of D to A will be again

* The substance of the lecture is here reprinted from 'Nature' of June 21 and 28, with the kind permission of the Editor, and after some slight revision by the author.

closer. Continue to act on the same principle until a stage is reached when the contour last drawn is undistinguishable from A. Suppose it to be the fourth stage; then as $2^4 = 16$, there are sixteen grades of least-discernible differences between A and B. If one of the contours differs greatly in a single or few respects from the other, reservation may be made of those peculiarities. Thus, if A has a deep notch in its lower right-hand border, we might either state that fact, and say that in other respects it differed from B by only 16 grades of unlikeness, or we might make no reservation, and continue subdividing until all trace of the notch was smoothed away. It is purely a matter of convenience which course should be adopted in any given case. The measurement of resemblance by units of least-discernible differences is applicable to shades, colours, sounds, tastes, and to sense-indications generally. There is no such thing as infinite unlikeness, because the number of just discernible difference between any objects, however dissimilar, is always finite. A point as perceived by the sense of sight is not a mathematical point, but an object so small that its shape ceases to be discernible. Mathematically, it requires an infinitude of points to make a short line; sensibly, it requires a finite and not a large number of what the vision reckons as points, to do so. If from thirty to forty points were dotted in a row across the disk of the moon, they would appear to the naked eyes of most persons as a continuous line.

Description within Specified Limits.—It is impossible to verbally define an irregular contour with such precision that a drawing made from the description shall be undistinguishable from the original, but we may be content with a lower achievement. Much would be gained if we could refer to a standard collection of contours drawn with double lines, and say that the contour in question falls between the double lines of the contour catalogued as number so-and-so. This would at least tell us that none of the very many contours that fell outside the specified limits could be the one to which the description applied. It is an approximate and a negative method of identification. Suppose the contour to be a profile, and for simplicity's sake let us suppose it to be only the portion of a profile that lies below the notch that separates the brow from the nose, and extending only so far downwards as the parting between the lips. Suppose it also to be the mere outline of a shadow sharply cast upon the wall by a single source of light, such as is excellently seen when a person stands sideways between the electric lantern and the screen in a lecture-room. All human profiles of this kind, when they have been reduced to a uniform vertical scale, fall within a small space. I have taken those given by Lavater, which are in many cases of extreme shapes, and have added others of English faces, and find that they all fall within the space shown in Fig. 2. The outer and inner limits of the space are of course not the profiles of any real faces, but the limits of many profiles, some of which are exceptional at one point, and others at another. We can classify the great majority of profiles so that

each of them shall be included between the double borders of one, two, or some small number of standard portraits, such as Fig. 3. I am as yet unprepared to say how near together the double borders of such standard portraits should be drawn; in other words, what is the smallest number of grades of unlikeness that we can satisfactorily deal with. The process of sorting profiles into their proper classes and of gradually building up a well-selected standard collection, is a laborious undertaking if attempted by any obvious way, but I believe it can be effected with comparative ease on the basis of measurements, as will be explained later on, and by an apparatus that will be described.

FIG. 2.

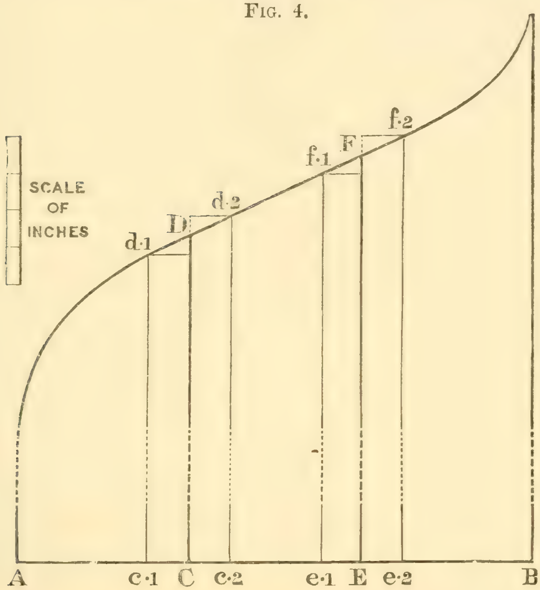
FIG. 3.

Classification of Sets of Measures.—Prisoners are now identified in France by the measures of their heads and limbs, the set of measures of each suspected person being compared with the sets that severally refer to each of many thousands of convicts. This idea, and the practical application of it, is due to M. Alphonse Bertillon. The actual method by which this is done is not all that could be theoretically desired, but it is said to be effective in action, and enables the authorities quickly to assure themselves whether the suspected person is or is not an old malefactor. The primary measures in the classification are four—namely, the head length, head breadth, foot length, and middle-finger length of the left foot and hand respectively. Each of these is classified according as it is large, medium or small. There are thus three, and only three, divisions of head lengths, each of which is subdivided into three divisions of head breadth; again, each of these is further subdivided into three of foot length, and these again into three of middle-finger length; thus the number of primary classes is equal to three multiplied into itself four times—that is to say, their number is eighty-one, and a separate pigeon-hole is assigned to each. All the exact measures and other notes on each criminal are written on the same card, and this card is stored in its appropriate pigeon-hole. The contents of each pigeon-hole are themselves sub-sorted on the same principle of three-fold classification in respect to other measures. This process can, of course, be extended indefinitely, but how far it admits of being carried on advantageously is another question. The fault of all hard-and-fast lines of classification, when variability is continuous, is the doubt where to place and where to look for values that are near the limits between two adjacent classes. Let us take Stature as an illustration of what must occur in every case, and let us represent its distribution by what I have called a "Scheme," as shown in Fig. 4.

Here the statures of any large group of persons are represented by lines of proportionate length. The lines are arranged side by side at equal distances apart on a base, A B, of convenient length.

A curve drawn through their tops gives the upper boundary of the scheme; the lines themselves are then wiped out, having served their purpose. If the base A B be divided into three equal parts and perpendiculars, C, D; E, F, be erected at the divisions between them, reaching from the base up to the curve, then the lengths of those

Fig. 4.



perpendiculars will be proportionate to the limiting values between the small and the medium group, and between those of the medium and the large group, respectively. The difference between these perpendiculars in the case of stature is about 2·3 inches. In other words, the shortest and tallest men in the medium class differ only by that amount. We have next to consider how much ought reasonably to be allowed for error of measurement. Considering that a man differs in height by a full third of an inch between the time of getting up in the morning and lying down at night; considering also that measures are recorded to the nearest tenth of an inch at the closest, also the many uncertainties connected with the measurement of stature, it would be rash not to allow for a possible (I do not say "probable") error of at least ± half an inch. Prolong C D, and note the points upon it at the distance of half an inch above and below D; draw horizontal lines from those points to meet the curve at *d*.1, *d*.2, and from the points of intersection drop perpendiculars reaching the base at *c*.1, *c*.2. A similar figure is drawn at F.

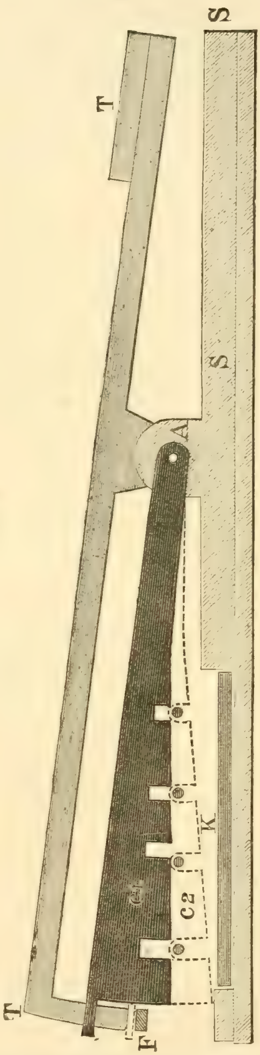
Then the ratio borne by the uncertain entries to the whole number of entries is as $c_1 c_2 + e_1 e_2$ to A B. This, as seen by the diagram, is a very large proportion. There is a dilemma from which those who adopt hard-and-fast lines of classification cannot escape: either the fringe of uncertainty must be dangerously wide, or else the delicacy with which measures are made cannot be turned to anything like its full account. If the delicacy is small, the fringe of uncertainty must be very wide; if the delicacy is great, the summed widths of all the fringes will be narrow, so long as there are only a few classes; but, on the other hand, by having only a few classes, most of the advantages of possessing delicate observations are wasted. The bodily measurements are so dependent on one another that we cannot afford to neglect small distinctions in an attempt to make an effective classification. Thus long feet and long middle-fingers usually go together. We therefore want to know whether the long feet in some particular person are accompanied by very long, or moderately long, or barely long fingers, though the fingers may in all three cases have been treated as long in M. Bertillon's system of classes, because they would be long as compared with those of the general population. Certainly his eighty-one combinations are far from being equally probable. The more numerous the measures the greater would be their interdependence, and the more unequal would be the distribution of cases among the various possible combinations of large, small, and medium values. No attempt has yet been made to estimate the degree of their interdependence. I am therefore having the above measurements (with slight necessary variation) recorded at my anthropometric laboratory for the purpose of doing so. This laboratory, I may add, is now open to public use under reasonable restrictions. It is entered from the Science Collections in the Western Galleries at South Kensington.

Mechanical Selector.—Feeling the advantage of possessing a method of classification that did not proceed upon hard and fast lines, I contrived an apparatus that is quite independent of them, and which I call a mechanical selector. Its object is to find which set, out of a standard collection of many sets of measures, resembles any one given set within any given degree of unlikeness. No one measure in any of the sets selected by the instrument can differ from the corresponding measure in the given set by more than a specified value. The apparatus is very simple; it applies to sets of measures of every description, and ought to act on a large scale as well as it does on a small one, with great rapidity, and be able to test several hundred sets by each movement. It relieves the eye and brain from the intolerable strain of tediously comparing a set of many measures with each of a large number of successive sets, in doing which a mental allowance has to be made for a *plus* or *minus* deviation of a specified amount in every entry. It is not my business to look after prisoners, and I do not fully know what need may really exist for new methods of quickly identifying suspected persons. If there be any real need, I should

think that this apparatus, which is contrived for other purposes, might, after obvious modifications, supply it.

The apparatus consists, in principle, of a large number of strips of card or metal *c* 1, *c* 2 (Fig. 5), say 8 or 9 inches long, and having a common axis A passing through all their smaller ends. A tilting-frame T, which turns on the same axis, has a front cross-bar F (whose section is seen in Fig. 5), on which the tips of the larger ends of all the cards rest whenever the machine is left alone. In this condition a counterpoise at the other end of T suffices to overcome the weight of all the cards, and this heavier end of T lies on the base-board S. When the heavy end of T is lifted, as shown in Fig. 5, its front-bar F is of course depressed, and the cards being individually acted on by their own weights, are free to descend with the cross-bar unless they are otherwise prevented. The lower edge of each card is variously notched to indicate the measures of the person it represents. Only four notches are shown in the figure, but six could be employed in a card of 8 or 9 inches long, allowing compartments of 1 inch in length to each of six different measures. The position of the notch in the compartment allotted to it, indicates the corresponding measure according to a suitable scale. When the notch is in the middle of a compartment, it means that the measure is of mediocre amount; when at one end of it, the measure is of some specified large value or of any other value above that; when at the other end the measure is of some specified small value or of any other value below it. Intermediate positions represent intermediate values according to the scale. Each of the cards corresponds to one of the sets of measures in the standard collection. The set of measures of the given person are indicated by the positions

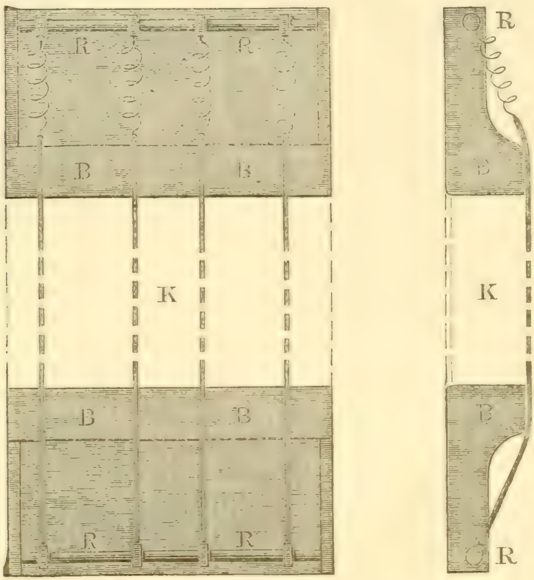
FIG. 5.



Section of the apparatus, but the bridges and rods are not shown, only the section of the wires.

of parallel strings or wires, one for each measure, that are stretched between Rods and across Bridges at either end of a long board set cross-ways to the cards. Their positions on the bridges are adjusted by the same scale as that by which the notches were cut in the cards. Figs. 6*a* and 6*b* are views of this portion of the apparatus, which acts as a key, and is of about 30 inches in effective length. The whole is shown in working position in Fig. 7. When the key is slid into its place, and the heavy end of the tilting-frame T is raised, all the cards are free to descend so far as the tilting-frame is concerned, but they

FIG. 6*a*. 6*b*.

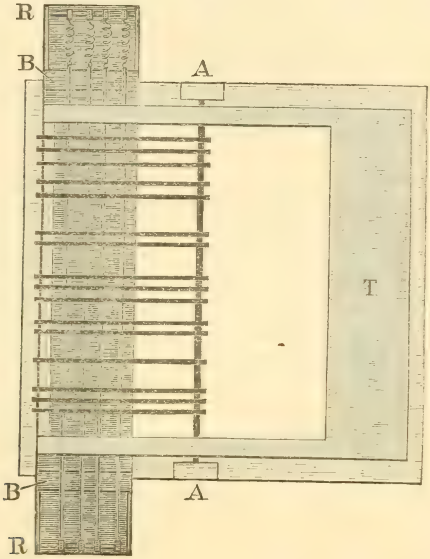


Plan and section of the key-board K.

are checked by one or more of the wires from descending below a particular level, except those few, if any, whose notches correspond throughout to the positions of the underlying wires. This is the case with the card *c*2 (Fig. 5), drawn with a dotted outline, but not with *c*1, which rests upon the third wire, counting from the axis. As the wires have to sustain the weight of all or nearly all the cards, frequent narrow bridges must be interposed between the main bridges to sustain the wires from point to point. The cards should be divided into batches by partitions corresponding to these interposed bridges, else they may press sideways with enough friction to interfere with their free independent action. Neither these interposed bridges nor the partitions are drawn in the figure. The method of adjusting the wires there shown,

is simply by sliding the rings to which they are attached at either end along the rod which passes through them. It is easy to arrange a more delicate method of effecting the adjustment if desired. Hitherto I have snipped out the notches in the cards with a cutter made on the same principle as that used by railway guards in marking the tickets of travellers. The width of the notch is greater than the width of the wire by an amount proportionate to the allowance intended to be made for error of measurement, and also for that

FIG. 7.



Reduced plan of complete apparatus.

Explanation.—A, the common axis; c_1, c_2, the cards; T, tilting-frame, turning on A (the cards rest by their front ends on F, which is the front cross-bar of T, at the time when the heavy hinder end of T rests on the base-board S); K is the key-board; R R are the rods between which the wires are stretched; B B are the bridges at either end of the key-board, over which the wires pass. (The explanation refers to the other figs., as well as to this.)

due to mechanical misfit. There seems to be room for 500 cards or metal strips, and ample room for 300 of them, to be arranged in sufficiently loose order within the width of 30 inches, and a key of that effective length would test all these by a single movement. It could also be applied in quick succession to any number of other collections.

Measurement of Profiles.—The sharp outline of a photographed profile admits of more easy and precise measurement than the yielding

outline of the face itself. The measurable distances between the profiles of different persons are small, but the available measures are much more numerous than might have been expected, and their variations are more independent of one another than those of the limbs. I suspect that measures of the profile may be nearly as trustworthy as those of the limbs for approximate identification, that is, for excluding a very large proportion of persons from the possibility of being mistaken for the one whose measurements are given. The measurement of a profile enables us to use a mechanical selector for finding those in a large standard collection to which they nearly correspond. From the selection thus made, the eye could easily make a further selection of those that suited best in other respects. A mechanical selector also enables us to quickly build up a standard collection step by step, by telling us whether or no each fresh set of measures falls within the limits of any of those already collected. If it does, we know that it is already provided for; if not, a new card must be added to the collection. There will be no fear of duplications, as every freshly-added standard will differ from all its predecessors by more than the specified range of permitted differences.

As regards the most convenient measurements to be applied to a profile for use with the selector, I am unable as yet to speak decidedly.

Fig. 8.

If we are dealing merely with a black silhouette, such as the shadow cast on a wall by a small and brilliant light, the best line from which to measure seems to be B C in Fig. 8; namely, that which touches both the concavity of the notch between the brow and nose, and the convexity of the chin. It is not difficult to frame illustrated instructions to explain what should be done in the cases where no line can be drawn that strictly fulfils these conditions. I have taken a considerable number of measures from the line that touches the brow and chin, but am now inclined to prefer that which I have just described. A sharp unit of measurement is given by the distance between this line and another drawn parallel to it just touching the nose, as at N in the figure. A small uncertainty in the direction of B C has but a very trifling effect on this distance. By dividing the interval between these parallel lines into four parts, and drawing a line through the third of the divisions, parallel to B C, we obtain the two important points of reference, M and R. M is a particularly well-defined point, from which O is determined by dropping a perpendicular from M upon B C. O seems the best of all points from which to measure. It is excellently placed for defining the shape and position of the notch between the nose and the upper lip, which is perhaps the most distinctive feature in the profile. O L can be determined with some precision; O B and O C are but coarse measurements.

In addition to these and other obvious measures, such as one or more to define the projection of the lips, it would be well to measure

the radius of the circle of curvature of the depression at B, also of that between the nose and the lip, for they are both very variable and very distinctive. So is the general slope of the base of the nose. The difficulty lies not in selecting a few measures that will go far towards negatively identifying a face, but in selecting the best—namely, those that can be most precisely determined, are most independent of each other, most variable, and most expressive of the general form of the profile. I have tried many different sets, and found all to be more or less efficient, but have not yet decided to my own satisfaction which to adopt.

We will now suppose that either by the above method or by any other, a standard collection of doubly outlined portraits such as that in Fig. 3, has been made and come into use, so that a profile can be approximately described by referring it to number so-and-so in the catalogue. If the number it contained was less than 1000, three figures would suffice to define any one of them. We will now consider how a yet closer description of the profile may be given by using a few additional figures. One way of doing so is to have short cross-lines drawn at critical positions between the two outlines of the standard, and to suppose each of them to be divided into eight equal parts. The intersection of the cross-lines with the outer border would count as 0; that with the inner border as 8, and the intermediate divisions from 1 to 7. As the cross-lines would be very short, a single numeral would thus define the position of a point in any one of them, with perhaps as much precision as the naked eye could utilise. By employing as many figures as there are cross-lines in the standard, each successive figure for each successive cross-line, a corresponding number of points in the profile would be fixed with great accuracy. Suppose a total of nine figures to be allowed, then the first three figures would specify the catalogue number of the portrait to be referred to, and the remaining six figures would determine six points in the outline of the portrait with greatly increased precision.

I may say that after numerous trials of different methods for comparing portraits successively by the eye, I have found none so handy and generally efficient as a double-image prism, which I largely used in my earlier attempts in making composite portraits.

I have not succeeded in contriving an instrument that shall directly compare a given profile with those in a standard collection, and which shall at the same time act with anything like the simplicity of the mechanical selector, and with the same quick decision in acceptance or rejection. Still, I recognise some waste of opportunity in not utilising the power of varying the depths of the notches in the cards, independently of their longitudinal position.

Personal characteristics exist in much more minute particulars than those just described. Leaving aside microscopic peculiarities, which are of unknown multitudes, such as might be studied in the 800,000,000

specimens cut by a microtome, say of one two-thousandth part of an inch in thickness, and one-tenth of an inch each way in area, out of the 4000 cubic inches or so of the flesh, fat, and bone of a single average human body, there are many that are visible with or without the aid of a lens.

The markings in the iris of the eye are of the above kind. They have been never adequately studied except by the makers of artificial eyes, who recognise thousands of varieties of them. These markings well deserve being photographed from life on an enlarged scale. I shall not dwell now upon these, nor on such peculiarities as those of handwriting, nor on the bifurcations and interlacements of the superficial veins, nor on the shape and convolutions of the external ear. These all admit of brief approximate description by the method just explained—namely, by reference to the number in a standard collection of the specimen that shall not differ from it by more than a specified number of units of unlikeness. I have already explained what is meant by a unit of unlikeness, and the mechanical means by which a given set of measures can be compared with great ease and by a single movement with every set simultaneously, in a large standard collection of sets of measures.

Perhaps the most beautiful and characteristic of all superficial marks are the small furrows, with the intervening ridges and their pores, that are disposed in a singularly complex yet regular order on the under surfaces of the hands and the feet. I do not now speak of the large wrinkles in which chiromantists delight, and which may be compared to the creases in an old coat, or to the deep folds in the hide of a rhinoceros, but of those fine lines of which the buttered fingers of children are apt to stamp impressions on the margins of the books they handle, that leave little to be desired on the score of distinctness. These lines are found to take their origin from various centres, one of which lies in the under surface of each finger-tip. They proceed from their several centres in spirals and whorls, and distribute themselves in beautiful patterns over the whole palmar surface. A corresponding system covers the soles of the feet. The same lines appear with little modification in the hands and feet of monkeys. They appear to have been carefully studied for the first time by Purkinje in 1822, and since then they have attracted the notice of many writers and physiologists, the fullest and latest of whom is Kollman, who has published a pamphlet, 'Tastapparat der Hand' (Leipzig, 1883), in which their physiological significance is fully discussed. Into that part of the subject I am not going to enter here. It has occurred independently to many persons to propose finger-marks as a means of identification. In the last century, Bewick, in one of the vignettes in the 'History of Birds,' gave a woodcut of his own thumb-mark, which is the first clear impression I know of, and afterwards one of his finger-marks. Some of the latest specimens that I have seen are by Mr. Gilbert Thomson, an officer of the American Geological Survey, who, being in Arizona, and having to make his orders for payment on a camp

suttler, hit upon the expedient of using his own thumb-mark to serve the same purpose as the elaborate scroll engraved on blank cheques—namely, to make the alteration of figures written on it impossible without detection. I possess copies of two of his cheques. A San Francisco photographer, Mr. Tabor, made enlarged photographs of the finger-marks of Chinese, and his proposal to employ them as a means of identifying Chinese immigrants, seems to have been seriously considered. I may say that I can obtain no verification of a common statement that the method is in actual use in the prisons of China. The thumb-mark has been used there as elsewhere in attestation of deeds, such as a man might make an impression with a common seal, not his own, and say, "This is my act and deed"; but I cannot hear of any elaborate system of finger-marks having ever been employed in China for the identification of prisoners. It was, however, largely used in India, by Sir William Herschel, many years ago, when he was an officer of the Bengal Civil Service. He found it to be most successful in preventing personation, and in putting an end to disputes about the authenticity of deeds. He described his method fully in 'Nature,' in 1880 (vol. xxiii. p. 76), which should be referred to; also a paper by Mr. Faulds in the next volume. I may also allude to articles in the American journal 'Science,' 1886 (vol. viii. pp. 166 and 212).

The question arises whether these finger-marks remain unaltered throughout the life of the same person. In reply to this I am enabled to submit a most interesting piece of evidence, which thus far is

Fig. 9.

Enlarged impressions of the fore and middle finger tips of the right hand of Sir William Herschel, made in the year 1860.

unique, through the kindness of Sir Wm. Herschel. It consists of the imprints of the two first fingers of his own hand, made in 1860 and in 1888 respectively, that is, at periods separated by an interval of twenty-eight years. I have also two intermediate imprints, made by him in 1874 and in 1883 respectively. Figs. 9 and 11 are cut from photographs on an enlarged scale of the imprints of 1860 and 1888, which were made direct upon the engraver's block; these woodcuts may therefore be relied on as very correct representations of the originals in my present possession. Fig. 10 refers to the portion of Fig. 9 to which I am about to draw attention. On first examining these and other finger-marks, the eye wanders and becomes confused, not knowing where to fix itself; the points shown in Fig. 10 are

Fig. 10.

Positions of furrow-heads and bifurcations of furrows, in Fig. 9.

Fig. 11.

Enlarged impressions of the fore and middle finger tips of the right hand of Sir William Herschel, made in the year 1888.

those it should select. They are the places at which each new furrow makes its first appearance. The furrows may originate in two principal ways, which are not always clearly distinguishable: (1) the new furrow may arise in the middle of a ridge; (2) a single furrow may bifurcate and form a letter Y. The distinction between (1) and (2) is not greatly to be trusted, because one of the sides of the ridge in case (1) may become worn, or be narrow and low, and not always leave an imprint, thus converting it into case (2); conversely case (2) may be converted into case (1). The position of the origin of the new furrow is, however, none the less defined. I have noted the furrow-heads and bifurcations of furrows in Fig. 9, and shown them separately in Fig. 10. The reader will be able to identify these positions with the aid of a pair of compasses, and he will find that they persist unchanged in Fig. 11, though there is occasionally uncertainty between cases (1) and (2). Also there is a little confusion in the middle of the small triangular space that separates two distinct systems of furrows, much as eddies separate the stream lines of adjacent currents converging from opposite directions. A careful comparison of Figs. 9 and 11 is a most instructive study of the effects of age. There is an obvious amount of wearing and of coarseness in the latter, but the main features in both are the same.

I happen to possess a very convenient little apparatus for examining finger-marks and for recording the positions of furrow-heads. It is a slight and small, but well-made wooden pentagraph, multiplying five-fold, in which a very low-power microscope, with *coarse* cross-wires, forms the axis of the short limb, and a pencil-holder forms the axis of the long limb. I contrived it for quite another use, namely, the measurement of the length of wings of moths in some rather extensive experiments that are now being made for me in pedigree moth-breeding. It has proved very serviceable in this inquiry also, and was much used in measuring the profiles spoken of in the last article. Without some moderate magnifying power the finger-marks cannot be properly studied. It is a convenient plan, in default of better methods, to prick holes with a needle through the furrow-heads into a separate piece of paper, where they can be studied without risk of confusing the eye. There are peculiarities often found in furrows that do not appear in these particular specimens, and to which I will not further refer. In Fig. 10 the form of the origin of the spirals is just indicated. These forms are various; they may be in single or in multiple lines, and the earlier turns may form long loops or be nearly circular. My own ten fingers show at least four distinct varieties.

Notwithstanding the experience of others to the contrary, I find it not easy to make clear and perfect impressions of the fingers. The proper plan seems to be to cover a flat surface, like that of a piece of glass or zinc, with a thin and even coat of paint, whether it be printers' ink or Indian ink rubbed into a thick paste, and to press the finger lightly upon it so that the ridges only shall become inked, then the

inked fingers are pressed on smooth and slightly damped paper. If a plate of glass be smoked over a paraffin lamp, a beautiful negative impression may be made on it by the finger, suitable for a lantern transparency. The blackened finger may afterwards be made to leave a positive impression on a piece of paper, that requires to be varnished if it is to be rendered permanent. All this is rather dirty work, but people do not seem to object to it; rivalry and the hope of making continually better impressions carry them on. It is troublesome to make plaster casts; modelling-clay has been proposed; hard wax, such as dentists use, acts fairly well; sealing-wax is excellent if the heat can be tolerated; I have some good impressions in it. For the mere study of the marks, no plan is better than that of rubbing a little thick paste of chalk ("prepared chalk") and water or sized water upon the finger. The chalk lies in the furrows, and defines them. They might then be excellently photographed on an enlarged scale. My own photographic apparatus is not at hand, or I should have experimented in this. When notes of the furrow-heads and of the initial shape of the spiral have been made, the measurements would admit of comparison with those in catalogued sets by means of a numerical arrangement, or even by the mechanical selector described in the last article. If a cleanly and simple way could be discovered of taking durable impressions of the finger tips, there would be little doubt of their being serviceable in more than one way.

In concluding my remarks, I should say that one of the inducements to making these inquiries into personal identification has been to discover independent features suitable for hereditary investigation. It has long been my hope, though utterly without direct experimental corroboration thus far, that if a considerable number of variable and independent features could be catalogued, it might be possible to trace kinship with considerable certainty. It does not at all follow because a man inherits his main features from some one ancestor, that he may not also inherit a large number of minor and commonly overlooked features from many ancestors. Therefore it is not improbable, and worth taking pains to inquire whether each person may not carry visibly about his body undeniable evidence of his parentage and near kinships.

[F. G.]

WEEKLY EVENING MEETING,

Friday, June 1, 1888.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

Professor J. A. Ewing, F.R.S. University College, Dundee.

Earthquakes and how to Measure them.

The lecturer pointed out that seismology was a science with two sides, one geological, the other mechanical. The geologist attacked the subject by at once attempting to refer earthquakes to their source in the crumpling, tearing, or slipping of strata, in volcanic eruption, in the collapse or explosion of subterranean cavities. The mechanical student of earthquakes, on the other hand, concerned himself with the character of the motion that was experienced, and with the means by which an earthquake spread from point to point by the elastic vibration of rock and soil. His first business was to find out exactly how the ground moved during an earthquake, to determine by direct measurement the amount and direction of every successive displacement, and the velocity and rate of acceleration at every instant while the shaking went on. This was the problem of seismometry, and the lecture would deal with the solution of this problem, and with some of the results which had been obtained in the measurement of earthquakes in Japan. Earthquakes happened there with a frequency sufficient to satisfy the most exacting seismologist. It had been estimated that one or another part of the empire was shaken every day, and in Tokio, where the measurements had been made, there was an earthquake, on the average, about once a week.

Most early attempts to reduce the observing of earthquakes to an exact science had failed, because they were based on a wrong notion of what an earthquake was. It had been imagined that an earthquake consisted of a single isolated jerk, or of a few jerks, easily distinguishable from any minor oscillations that might accompany them. The old column seismometer, for instance, the use of which was recommended in the 'Admiralty Manual of Scientific Enquiry,' attempted to measure what was called the intensity of the shock by means of a number of columns of various diameters which stood like ninepins on a level base. It was expected that the shock would overthrow the narrower columns up to a breadth which would gauge the intensity of the disturbance, and also that the line in which they fell would show the horizontal direction of transit of the earthquake wave. In fact, however, such columns fell most capriciously when they fell at all. The reason was that in an earthquake there was no single outstanding impulse. There was a confused jumble of oscillations, very numerous, and very irregular, which shifted their direction

with such rapidity that a point on the earth's surface wriggled through a path like the form a loose coil of string might take if it were ravelled into a state of the utmost confusion. The mechanical problem in seismometry was to find a steady point, to suspend a body so that some point in it, at least, should not move while this complicated wriggling was going on; the steady point would then serve as a *datum* with respect to which the movement of the ground might be recorded and measured. The simple pendulum had often been suggested as a steady-point seismometer, but in the protracted series of oscillations which made up an earthquake the bob of a pendulum might, and often did, acquire so much oscillation that, far from remaining steady, it moved more than the ground itself.

The lecturer illustrated this by showing the cumulative effect of a succession of small impulses on a pendulum when these happened to agree in period with the pendulum's swing. The fault of the pendulum, from the seismometric point of view, was its too great stability, and its consequently short period of free oscillation. To prevent the body whose inertia was to furnish a steady point from acquiring independent oscillation, the body must be suspended or supported astatically; in other words, its equilibrium must be very nearly neutral. Methods of astatic suspension which had been used in seismometry were described and illustrated by diagrams and models, in particular the ball and block seismometer of Dr. Verbeck, the horizontal pendulum, and a method of suspension by cross cords based on the Tchebicheff straight-line link-work.*

The complete analysis of the ground's motion was effected by a seismograph which resolved it into three components; two horizontal and one vertical, and recorded each of these separately, with respect to an appropriate steady-point, by means of a multiplying lever, on a sheet of smoked glass which was caused to revolve at a uniform rate by clock-work. The clock was started into motion by the action of the earliest tremors of the earthquake on a very delicate electric seismoscope, the construction of which was shown by a diagram. In this way a record was deposited upon the revolving plate which gave every possible particular regarding the character of the earth's motion at the observing-station. A complete set of the instruments as now manufactured by the Cambridge Scientific Instrument Company was shown in action.† Professor Ewing also described his duplex pendulum seismograph, which draws on a fixed plate of smoked glass a magnified picture of the horizontal motion of the ground during an earthquake.‡ Apparatus was shown for testing the accuracy of the seismographs by means of imitation earthquakes, which shook the

* See a memoir on 'Earthquake Measurement,' by Professor Ewing, published by the University of Tokio, 1883. Also 'Proc. Roy. Soc.' No. 210, 1881; and 'Transactions of the Seismological Society of Japan,' from 1880.

† See 'Nature,' vol. xxxiv. p. 343.

‡ Trans. Seis. Soc. Jap. vol. v. p. 89, and vol. viii. p. 83; 'Encyclopædia Britannica,' Art. 'Seismometer'; 'Proc. Roy. Soc.,' June 21, 1888.

stand of the instrument, and drew two diagrams side by side upon the glass plate—one, the record given by the seismograph itself, and the other the record derived from a fixed piece which was held fast in an independent support. The agreement of the two records with one another proved how very nearly motionless the "steady-point" of the seismograph remained during even a prolonged shaking resembling an earthquake. This test was applied to the instruments on the table, and the close agreement of the two diagrams was exhibited by projecting them on the lantern-screen. A large number of autographic records of Japanese earthquakes were thrown on the screen,* and particulars were given of the extent of the motion, and the velocity and rate of acceleration, in some representative examples. To determine the rate of acceleration was of special interest, because it measured the destructive tendency of the shock.

The lecturer explained that some of the seismograms exhibited on the screen had been obtained since he had left Japan by his former assistant, Mr. Sekiya, who now held the unique position of Professor of Seismology in the Imperial Japanese University. Professor Sekiya had recently taken the pains to construct a model representing, by means of a long coil of copper wire carefully bent into the proper form, the actual path pursued by a point on the earth's surface during a prolonged and rather severe shaking. This model of an earthquake had been made by combining the three components of each successive displacement as these were recorded by a set of seismographs like those upon the lecture-table. The appearance of Professor Sekiya's model (a description of which will be found in 'Nature,' vol xxxvii. p. 297) was shown to the audience by means of the lantern.

Professor Ewing drew attention to the small tremors of high frequency which characterised the beginnings of earthquake motion, and which were apparent in a number of the diagrams he exhibited. These generally disappeared at a comparatively early stage in the disturbance. In the early portion they were as a rule found at first alone, preceding the larger and slower principal motions; and then when the principal motions began, small tremors might still be seen for some time, superposed upon them. In all probability these quick-period tremors were normal vibrations, while the larger motions were transverse vibrations; and a reference to the theory of the transmission of vibrations in elastic solids served to explain why the quick-period tremors were the first to be felt. The whole disturbance went on for several minutes, with irregular fluctuations in the amplitude of the motion, and with a protracted dying out of the oscillations, the period of which usually lengthened towards the close. The record of a single earthquake comprised some hundreds of successive movements, to and fro, or round fantastic loops. Each single movement usually occupied from half a second to two seconds. Earthquakes

* Examples of these will be found in the lecturer's memoir on 'Earthquake Measurement,' also 'Nature,' vol. xxx. p. 174, xxxi p. 581, xxxvi. p. 107.

were quite perceptible in which the greatest extent of motion was no more than $\frac{1}{100}$ of an inch. In one case, on the other hand, Professor Sekiya had obtained a record in which the motion was as much as an inch and three-quarters. Even that was in an earthquake which did comparatively little damage, and there was therefore reason to expect that in a severely destructive shock (such as had not occurred since the present system of seismometry was developed) the motion might be considerably greater.

Professor Ewing concluded his lecture by pointing out that the seismographs he had described might find practical application in measuring the stiffness of engineering structures. He exhibited, by the lantern, seismographic records he had recently taken on the new Tay Bridge, to examine the shaking of the bridge during the passage of trains. The instrument had been placed on the southernmost of the greater girders, where there was reason to expect the vibration would be a maximum. The extent of motion was remarkably small. It was less than an eighth of an inch, even while the train was passing the seismograph—a fact which spoke well for stiffness of the structure. Nevertheless, by watching the index of the seismograph he had been able to tell whenever a train came on at the Dundee end of the bridge, a distance of $1\frac{1}{3}$ mile from the place where the instrument was standing. One could then detect a vibratory motion, the extent of which was probably not more than $\frac{1}{500}$ of an inch. This began in the longitudinal direction, and for some time longitudinal vibration only could be seen. As the train came nearer lateral vibration also began, and the amplitude of course increased. It reached a maximum when the train was close to the seismograph, and continued visible until the train had passed off the bridge at the other end.*

[J. A. E.]

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* Particulars of these experiments have been communicated to the Royal Society, and will be found in the 'Proceedings' for June 21, 1888.

GENERAL MONTHLY MEETING,

Monday, June 4, 1888.

His Grace The Duke of Northumberland, K.G. D.C.L. LL.D. President, in the Chair.

F. W. Bayley, Esq. F.C.S.
Jacob Feis, Esq.
Charles Albert Flint, Esq.
Arthur Holland, Esq.
Mrs. John Mackinlay.
Thomas Woolner, Esq. R.A.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Professor W. Chandler Roberts-Austen, *M.R.I.* for his present of a Portable Assay Furnace.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Governor-General of India—Geological Survey of India : Palæntologia Indica, Ser. XIII. Vol. I. Part 7. 4to. 1887.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta : Rendiconti. Vol. IV. 1e Semestre, Fasc. 2–4. 8vo. 1888.
Memorie della Classe di Scienze Morali Storiche e Filologiche. Serie 3a, Vol. XII. 4to. 1884. Atti, 1875–6. 2a Serie, Vol. IV. 4to. 1887.
Academy of Natural Sciences, Philadelphia—Proceedings, 1887, Part 3. 8vo. 1887.
Agricultural Society of England, Royal—Journal, Second Series, Vol. XXIV. Part 1. 8vo. 1888.
Asiatic Society of Bengal—Descriptions of New Indian Lepidopterous Insects. By F. Moore. Part 3. 4to. 1888.
Astronomical Society, Royal—Monthly Notices, Vol. XLVIII. No. 6. 8vo. 1888.
Boston Society of Natural History—Memoirs, Vol. IV. Nos. 1–4. 4to. 1886–8.
British Architects, Royal Institute of—Proceedings, 1887-8, No. 14. 4to.
Chemical Society—Journal for May, 1888. 8vo.
Churchill, Messrs. J. and A. (the Publishers)—Journal of Laryngology and Rhinology, Vol. II. No. 5. 8vo. 1888.
Civil Engineers' Institution—Minutes of Proceedings. Vol. XCII. 8vo. 1887–8.
Davies, G. C. Esq. (the Author)—Handbook to the Rivers and Broads of Norfolk and Suffolk. 9th edition. 12mo. 1887.
East India Association—Journal, 1888, No. 2. 8vo.
Editors—American Journal of Science for May, 1888. 8vo.
Analyst for May, 1888. 8vo.
Athenæum for May, 1888. 4to.
Chemical News for May, 1888. 4to.
Chemist and Druggist for May, 1888. 8vo.
Engineer for May, 1888. fol.
Engineering for May, 1888. fol.
Horological Journal for May, 1888. 8vo.
Industries for May, 1888. fol.
Iron for May, 1888. 4to.
Murray's Magazine for May, 1888. 8vo.
Nature for May, 1888. 4to.
Revue Scientifique for May, 1888. 4to.
Scientific News for May, 1888. 4to.
Telegraphic Journal for May, 1888. 8vo.
Zoophilist for May, 1888. 4to.
Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 56, 57. 8vo. 1888.
Indici e Cataloghi, Vol. I. Fasc. 7. 8vo. 1888.
Franklin Institute—Journal, No. 749. 8vo. 1888.
Geneva : Société de Physique et d'Histoire Naturelle—Memoires, Tome XXIX. Partie 2. 4to. 1886–7.
Geographical Society, Royal—Proceedings, New Series, Vol. X. No. 5. 8vo. 1888.
Geological Institute, Imperial, Vienna—Verhandlungen, 1888, No. 5. 8vo.
Geological Society—Quarterly Journal, No. 174. 8vo. 1888.
Georgofili, Reale Accademia—Atti, Quarta Serie, Vol. XI. Disp. 1. 8vo. 1888.
Johns Hopkins University—University Circular, No. 65. 4to. 1888.

Leighton, John, Esq. F.S.A. M.R.I. (the Author)—A System of Ballot. 12mo. 1888.
Medical and Chirurgical Society, Royal—Proceedings, No. 18. 8vo. 1888.
Meteorological Office—Weekly Weather Reports, Vol. V. Nos. 8–18. 4to. 1888. Monthly Weather Reports, March, April, 1887. 4to.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quinta, Vol. II. No. 2. 8vo. And Disegni. fol. 1888.
Musical Association—Proceedings, 10th to 13th Sessions, 1883–7. 8vo.
National Life-Boat Institution, Royal—Annual Report, 1888. 8vo.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXVII. Parts 3, 4. 8vo. 1888.
Odontological Society of Great Britain—Transactions, Vol. XX. Nos. 6, 7. New Series. 8vo. 1888.
Pharmaceutical Society of Great Britain—Journal, May, 1888. 8vo.
Photographic Society—Journal, Vol. XII. No. 7. 8vo. 1888.
Physical Society of London—Proceedings, Vol. IX. Part 2. 8vo. 1888.
Richardson, B. W. M.D. F.R.S. (the Author)—The Asclepiad, Vol. V. No. 18. 8vo. 1888.
Royal Society of London—Proceedings, Nos. 264–266. 8vo. 1888.
Smithsonian Institution—Smithsonian Miscellaneous Collections, Vol. XXXI. 8vo. 1888.
Society of Arts—Journal, May, 1888. 8vo.
Surgeon-General's Office, U.S. Army—Index-Catalogue of the Library. Vols. 7 and 8. 4to. 1886–7.
Sydney Morning Herald—History of Australian Settlement and Progress, with Reports of Centennial Celebrations in 1888. 4to.
Telegraph Engineers, Society of—Journal, No. 72. 8vo. 1888.
United Service Institution, Royal—Journal, No. 143. 8vo. 1888.
Upsal University—Bulletin Mensuel de l'Observatoire Météorologique, Vol. XIX. 4to. 1887–8.
Vereins zur Beförderung des Gewerbfleises in Preussen—Verhandlungen, 1888: Heft 4. 4to.
Victoria Institute—Transactions, No. 84. 8vo. 1888.
Wimshurst, James, Esq. M.R.I. (the Author)—Electric Influence Machines. 4to. 1888.

WEEKLY EVENING MEETING,

Friday, June 8, 1888.

Sir Frederick Bramwell, D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Professor Dewar, M.A. F.R.S. *M.R.I.*

Phosphorescence and Ozone.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, May 11, 1888.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

Professor W. Chandler Roberts-Austen, F.R.S. *M.R.I.*

Some curious Properties of Metals and Alloys.

The lecture consisted mainly of experimental demonstrations of the changes induced in metals, either by slight variations in the treatment to which they are subjected or by rendering them impure by the addition of small quantities of metals or metalloids.

Prof. Austen began by pointing out that for centuries the early metallurgists investigated the action of exceedingly small quantities of matter upon masses of metal, and he said that, strange as it may seem, the promulgation, in 1803, of Dalton's atomic theory threw a flood of light upon chemical phenomena, but cast into shade such investigations as those of Bergman which dealt with influences of "traces" upon masses, and the authority of Berthollet was not sufficient to save them from neglect. In this eventful year for science, 1803, the latter published his essay on chemical statics, in which he stated, as a fundamental proposition, that in comparing the action of bodies on each other, which depends "upon their affinities and mutual proportions, the mass of each has to be considered." * His views were successfully contested by Proust, but, as Lothar Meyer says, the influence on chemistry of the rejection of Berthollet's views was remarkable: "All phenomena which could not be attributed to fixed atomic proportions were set aside as not truly chemical, and were neglected. Thus chemists forsook the bridge by which Berthollet had sought to unite the sister sciences, physics and chemistry." Fortunately, however, in this country there was one chemist who had followed up the line of work indicated by the early metallurgists, for in 1803, the same year as that in which both Berthollet's essay and Dalton's atomic theory were published, Charles Hatchett † communicated to the Royal Society the results of a research which he had conducted, with the assistance of Cavendish, in order to ascertain "the chemical effects produced on gold by different metallic substances when employed in certain" (often very small) "proportions as alloys."

Allusion was then made to the evidence of the passage of metals into allotropic states, and it was shown that although the importance of the isomeric and allotropic states was abundantly recognised in organic chemistry, it had been much neglected in the case of metals.

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* English edition (by M. Farrell, M.D.), 1804, p. 5.
† Phil. Trans. vol. xciii. p. 43, 1803.

Special attention was then devoted to the work of Joule and Lyon Playfair, who showed, in 1846, that metals in different allotropic states possessed different atomic volumes, and the lecturer then proceeded to the consideration of the work of Matthiessen who, in 1860, was led to the view that in certain cases when metals were alloyed, they passed into allotropic states, probably the most important generalisation which has as yet been made in connection with the molecular constitution of alloys.

Instances of allotropy in pure metals were then shown to the audience, such, for example, as Bolley's lead which oxidises readily in air ; Schützenberger's copper ; Fritsche's tin, which fell to powder when exposed to an exceptionally cold winter ; Gore's antimony; Graham's palladium ; and allotropic nickel. It was further shown that metals could be obtained in chemically active states under the following conditions :—Joule proved that when iron is released from its amalgam by distilling away the mercury the metallic iron takes fire on exposure to air, and is therefore clearly different from ordinary iron, and is, in fact, an allotropic form of iron. Moissan * has shown that similar effects are produced in the case of chromium and manganese, cobalt, and nickel, when released from their amalgams with mercury.

Evidence is not wanting of allotropy in metals released from solid alloys, as well as from fluid amalgams with mercury. Certain alloys may be viewed as solidified solutions, and when such bodies are treated with a suitable solvent, usually an acid, it often happens that one constituent metal is dissolved, and the other released in an insoluble form. Reference was then made to a new alloy of potassium and gold, containing about ten per cent. of the precious metal. If a fragment of this alloy be thrown upon water, the potassium takes fire, decomposes the water, and the gold is released as a black powder ; there is a form of this black or dark-brown gold which appears to be an allotropic modification of gold, as it combines with water to form auric hydride. By heating this dark gold to dull redness, it readily assumes the ordinary golden colour. The Japanese use this gold, released from gold-copper alloys, in a remarkable way, for they produce, by the aid of certain pickling solutions, a beautiful patina on copper which contains only two per cent. of gold, while even a trace of the latter metal is sufficient to alter the tint of the patina.

With regard to theoretical views as to molecular change in metals, special care was given to a description of the work of Professor W. Spring, of Liége, who had furnished much evidence in support of the view that polymerization of metals, that is the rearrangement of atoms in their molecules, could take place even in *solid* alloys of lead and tin.

With reference to the passage of metals into allotropic states under slight external influences, it was stated that Debray † has

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* Comptes Rendus, vol. lxxxviii. p. 180, 1879.
† Ibid. vol. xc. p. 1195, 1880.

given a case of an alloy in which a simple elevation of temperature induces allotropic change in the constituent metals. It is prepared as follows: ninety-five parts of zinc are alloyed by fusion with five parts of rhodium, and the alloy is treated with hydrochloric acid, which dissolves away the bulk of the zinc, leaving a rich rhodium-zinc alloy, containing about 80 per cent. of rhodium. When this alloy is heated in vacuo to a temperature of 400° C., a slight explosion takes place, but no gas is evolved, and the alloy is then insoluble in *aqua regia*, which dissolved it readily before the elevation of temperature caused it to change its state. We are thus presented (as the experiment shown to the audience proved) with another undoubted case of isomerism in alloys, the unstable, soluble modification of the alloy being capable of passing into the insoluble form by a comparatively slight elevation of temperature.

The industrial importance of the passage of metals and alloys into allotropic states, and the possibility of changing the mechanical properties of metals by apparently slight influences, was fully dealt with, and the lecture concluded with a detailed description of Professor Austen's own experiments which have since been printed in the 'Philosophical Transactions' of the Royal Society, the results showing that very small amounts of metallic impurities exert an extraordinary effect on the tenacity and extensibility of gold, and that small as the amounts of these impurities are, their influence is rigidly controlled by the Periodic Law of Newlands and Mendeléef, the deleterious action of a metallic impurity being in direct relation to its atomic volume. The audience was asked "to remember that the knowledge of the kind of facts which had been considered comes to us from very early times, for the influence produced on metals by small quantities of added matter had a remarkable effect on the development of chemistry, mainly by sustaining the belief of the early chemists in the possibility of ennobling a base metal so as to transmute it into gold. This was the object to which they devoted life and health, and laboured with fast and vigil. We inherit the results of their labours, and their prayers have been answered in a way they little anticipated, for, from an industrial point of view, if not from a scientific one, metals are "transmuted" by traces of impurity. Possibly we are nearing an explanation of the causes which are at work, but the fact remains that iron may be changed from a plastic material, which in ornament can be fashioned into the most dainty lines of flow, into one of great endurance, to which, for the present at least, the defence of the country may be trusted, apparently because armour-plates and missiles owe their respective qualities to the fact that carbon, manganese, and chromium have small atomic volumes."

GENERAL MONTHLY MEETING,

Monday, July 2, 1888.

JOHN RAE, M.D. LL.D. F.R.S. Vice-President, in the Chair.

A. Gordon Salamon, Esq. F.C.S. F.I.C.
Thomas Graham Young, Esq. F.R.S.E.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research:—

Ludwig Mond, Esq.	£100
John Bell Sedgwick, Esq.	25
Mrs. Bell Sedgwick	25

The Special Thanks of the Members were returned to Sir William Thomson for his valuable present of a set of three electric current measuring instruments (1) Magnetostatic milli-ampere meter; (2) Standard deci-ampere balance; (3) Magnetostatic deka-ampere meter.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Reale, Roma—Attie, Serie Quarta: Rendiconti. 1° Semestre, Vol. IV. Fasc. 5, 6. 8vo. 1888.
American Association for the Advancement of Science—Proceedings, 36th Meeting, New York, 1887. 8vo. 1888.
Antiquaries, Society of—Archæologia, Vol. LI. Part 1. 4to. 1888.
Proceedings, Vol. XII. No. 1. 8vo. 1888.
Asiatic Society, Royal (China Branch)—Journal, Vol. XXII. Nos. 3, 4. 8vo. 1887.
Astronomical Society, Royal—Monthly Notices, Vol. XLVIII. No. 7. 8vo. 1888.
Bankers' Institute—Journal, Vol. IX. Part 6. 8vo. 1888.
British Architects, Royal Institute of—Proceedings, 1887–8, Nos. 15, 16, 17. 4to.
Buffalo Library—The Buffalo Library and its Building. With Views. 4to. 1887.
Chemical Society—Journal for June, 1888. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, 1888, Part 3. 8vo.
Cutter, Ephraim, Esq.—The Clinical Morphologies. 8vo. 1888.
Dax: Société de Borda—Bulletin, Treizième Année, 2e Trimestre. 8vo. 1888.

Editors—American Journal of Science for June, 1888. 4to.
Analyst for June, 1888. 8vo.
Athenæum for June, 1888. 4to.
Chemical News for June, 1888. 8vo.
Chemist and Druggist for June, 1888. 8vo.
Engineer for June, 1888. fol.
Engineering for June, 1888. fol.
Horological Journal for June, 1888. 8vo.
Industries for June, 1888. fol.
Iron for June, 1888. 4to.
Murray's Magazine for June, 1888. 8vo.
Nature for June, 1888. 4to.
Revue Scientifique for June, 1888. 4to.
Scientific News for June, 1888. 4to.
Telegraphic Journal for June, 1888. 8vo.
Zoophilist for June, 1888. 4to.

Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 58, 59. 8vo. 1888.
Franklin Institute—Journal, No. 750. 8vo. 1888.
Geneva: Société de Physique et d'Histoire Naturelle—Memoires, Tome XXX. No. 4. 4to. 1888.
Geographical Society, Royal—Proceedings, New Series, Vol. X. No. 6. 8vo. 1888.
Geological Institute, Imperial, Vienna—Verhandlungen, 1888, Nos. 7, 8. 8vo.
Harlem, Société Hollandaise des Sciences—Œuvres Complètes de Christiaan Huygens. Tome I. Correspondence, 1638–56. 4to. 1888.
John Hopkins University—Taxation in American States and Cities. By R. T. Ely. 8vo. 1888.
Linnean Society—Journal, No. 155. 8vo. 1888.
Manchester Geological Society—Transactions, Vol. XIX. Parts 18, 19. 8vo. 1888.
Manchester Steam Users' Association—Boiler Explosions Act, 1882. Board of Trade Reports, Nos. 187–223. fol. 1886–7.
Meteorological Office—Quarterly Weather Reports, 1879, Part 3. 4to. 1888.
Hourly Reading, 1885, Part 3. 4to. 1888.
Charts showing the Mean Barometrical Pressure over the Atlantic, Indian, and Pacific Oceans. 1888.
Meteorological Society, Royal—Quarterly Journal, No. 66. 8vo. 1888.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quinta, Vol. II. No. 3. 8vo. And Disegni. fol. 1888.
Newall, Major-General D. J. F. R.A. (the Author)—The Highlands of India. 8vo. 1882.
New York Academy of Sciences—Transactions, Vol. VII. Nos. 1, 2. 8vo. 1887–8.
Annals, Vol. IV. Nos. 3 and 4. 8vo. 1888.
Pennsylvania Geological Survey—Annual Report, 1886, Part III. With Atlas. 8vo. 1887. Western Middle Atlas, Part 2. 8vo. 1887. Atlas, C 7. 8vo. 1887.
Pharmaceutical Society of Great Britain—Journal, June, 1888. 8vo.
Photographic Society—Journal, Vol. XII. No. 8. 8vo. 1888.
Preussische Akademie der Wissenschaften—Sitzungsberichte, I.–XX. 8vo. 1888.
Royal Society of London—Proceedings, No. 267. 8vo. 1888.
Royal Society of New South Wales—Journal and Proceedings, Vol. XXI. 8vo. 1888.
Society of Arts—Journal, June, 1888. 8vo.
Telegraph Engineers, Society of—Journal, No. 73. 8vo. 1888.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1888. Heft 5. 4to.
Zoological Society of London—Proceedings, 1888, Part I. 8vo. 1888.

GENERAL MONTHLY MEETING,

Monday, November 5, 1888.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Vice-President, in the Chair.

Amand Routh, M.D.

was elected a Member of the Royal Institution.

The Special Thanks of the Members were returned for the following Donation to the Fund for the Promotion of Experimental Research:—

Lachlan Mackintosh Rate, Esq. £50

The Special Thanks of the Members were returned to Messrs. Crossley for their valuable present of one of their Gas Engines (4 horse-power).

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Observations for 1886. 4to. 1888.
Greenwich Spectroscopic and Photographic Results, 1886, 1887. 4to. 1886–7.
Annals of the Cape Observatory, Vol. II. Part 2. 4to. 1888.
Cape Meridian Observations, 1882–1884. 8vo. 1887.
The Governor-General of India—Geological Survey of India: Records, Vol. XXI. Parts 2, 3. 8vo. 1888.
The Secretary of State for India—Great Trigonometrical Survey of India, Vol. X. 4to. 1887.
Catalogue of the Library of the India Office. 2 vol. 8vo. 1888.
Meteorological Observations at Simla, 1841-5, Vol. II. 4to. 1877.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta; Rendiconti. 1° Semestre, Vol. IV. Fasc. 7–12. 8vo. 1888.
Memorie della Classe di Scienze Morali Storiche e Filologiche. Serie 3ª, Vol. XII. 4to. 1884. Atti, 1887. 4ª Serie, Vol. III. 4to. 1887.
Academy of Natural Sciences, Philadelphia—Proceedings, 1888, Part 1. 8vo. 1888.
American Philosophical Society—Proceedings, No. 127. 8vo. 1888.
Aristotelian Society—Proceedings, Vol. I. No. 1. 8vo. 1888.
Asiatic Society of Bengal—Journal, Vol. LVI. Part 2, No. 4; Vol. LVII. Part 2, No. 1. 8vo. 1888.
Proceedings, 1888, Nos. 2, 3. 8vo.
Asiatic Society, Royal (China Branch)—Journal, Vol. XXII. No. 5. 8vo. 1888.

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GENERAL MONTHLY MEETING,

Monday, December 3, 1888.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S. Vice-President, in the Chair.

William Dunsmore Bohm, Esq.
Rev. Henry Thomas Cart, M.A.
Mrs. Charles Daniell,
Josiah Goodall, Esq.
Miss C. Naden,
Benjamin Ward Richardson, M.D. F.R.S.
Colonel T. E. Tennant,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned for the following Donations to the Fund for the promotion of Experimental Research:

Mrs. R. J. Mann (for the late Dr. R. J. Mann, M.R.I.) ..	£20
Mrs. Bloomfield Moore	£50
Warren De la Rue, Esq.	£100

The following Lecture Arrangements were announced:—

PROFESSOR DEWAR, M.A. F.R.S. *M.R.I.* Fullerian Professor of Chemistry, R.I. Six Lectures (adapted to a Juvenile Auditory) on CLOUDS AND CLOUDLAND. On Dec. 27 (*Thursday*), Dec. 29, 1888; Jan. 1, 3, 5, 8, 1889.

GEORGE JOHN ROMANES, Esq. M.A. LL.D. F.R.S. *M.R.I.* Fullerian Professor of Physiology, R.I. Twelve Lectures, constituting the second part of a Course on BEFORE AND AFTER DARWIN (The Evidences of Organic Evolution, and the Theory of Natural Selection). On *Tuesdays*, Jan. 22 to April 9.

PROFESSOR J. W. JUDD, F.R.S. Four Lectures on THE METAMORPHOSES OF MINERALS. On *Thursdays*, Jan. 24, 31, Feb. 7, 14.

SIDNEY MARTIN, M.D. F.R.C.S.E. B.Sc. Four Lectures on THE VENOM OF SERPENTS AND ALLIED POISONS, INCLUDING THOSE USED IN THE MIDDLE AGES. On *Thursdays*, Feb. 21, 28, March 7, 14.

J. HENRY MIDDLETON, Esq. M.A. Slade Professor of Fine Art in the University of Cambridge. Four Lectures on HOUSES AND THEIR DECORATION FROM THE CLASSICAL TO THE MEDIEVAL PERIOD. On *Thursdays*, March 21, 28, April 4, 11.

PROFESSOR ERNST PAUER. Four Lectures on THE CHARACTER OF THE GREAT COMPOSERS AND THE CHARACTERISTICS OF THEIR WORKS (with Illustrations on the Pianoforte). On *Saturdays*, Jan. 26, Feb. 2, 9, 16.

THE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. *M.R.I.* Professor of Natural Philosophy, R.I. Eight Lectures on EXPERIMENTAL OPTICS (POLARIZATION; WAVE THEORY). On *Saturdays*, Feb. 23 to April 13.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1e Semestre, Vol. IV. Fasc. 13; 2o Semestre, Vol. IV. Fasc. 2, 3, 4, 5. 8vo. 1888.
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Amsterdam Société Royale de Zoologie—Bydragen tot de Dierkunde. Afl. 14, 15, 16. 4to. 1888.
Feestnummer. 4to. 1888.
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Proceedings, 1888, Nos. 4–8. 8vo.
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Harlem, Société Hollandaise des Sciences—Archives Neerlandaises, Tome XXIII. Liv. 1. 8vo. 1888.
Longstaff, Ll. W. Esq. F.R.G.S. M.R.I. (the Author)—Notes on the Wimbledon Free Public Library. 8vo. 1888.
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Catalogue of Coins, No. 2. By E. Thurston. 8vo. 1888.
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Meteorological Observations at Stations of the Second Order for 1884. 4to. 1888.
Contributions to the Knowledge of the Meteorology of the Arctic Regions. Part 5. 4to. 1888.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quinta. Vol. II. Nos. 7, 8. 8vo. And Disegni. fol. 1888.

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Perry, Rev. S. J. F.R.S. (the Author)—Results of Meteorological and Magnetical Observations, 1887. 12mo. 1888.

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Royal Institution of Cornwall—Journal, Vol. IX. Parts 1, 2, 3. 8vo. 1886–8.

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Catalogue de la Bibliothèque. Liv. 7, 8. 4to. 1887–8.

United Service Institution, Royal—Journal, No. 145. 8vo. 1888.

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 25, 1889.

COLONEL J. A. GRANT, C.B. C.S.I. F.R.S. Vice-President. in the Chair.

PROFESSOR G. H. DARWIN, M.A. LL.D. F.R.S. *M.R.I.*

*Meteorites and the History of Stellar Systems.**

THE great advances which have been recently made in the art of celestial photography have now made it possible to study the details of structure of some of the nebulæ, which formerly only appeared as a chaotic luminosity, even when viewed through a powerful telescope. To illustrate this new method of research, a photograph of the great nebula in Andromeda, by Mr. Isaac Roberts, was exhibited; it showed that the nebula consists of a bright central condensation, surrounded by several faintly luminous concentric rings.

A short sketch of the Nebular Hypothesis of Laplace and Kant was then given. This is a mechanical theory of the evolution of a nebula into a star with attendant planets.

It was then pointed out that Mr. Roberts's photograph exhibits exactly the condition which Laplace imagined, and it thus confirms the substantial truth of his hypothesis.

But many points in the evolution of a planetary system are still involved in much obscurity, and there is in particular one difficulty, so fundamental that some astronomers have been led by it virtually to throw over the nebular hypothesis.

That theory attributes the annulation of the nebula to the gradual diminution and ultimate vanishing of gaseous pressure at the equator of a rotating mass of gas. Thus it is the very essence of the hypothesis that the nebula should consist of continuous gas.

Now there is in the solar system at present no trace of the all-pervading gas from which it is supposed to have been evolved, whilst there is much evidence that the space surrounding the sun and planets is peopled by countless loose stones or meteorites flying about in various directions.

This latter view is confirmed by the recent spectroscopic researches of Mr. Lockyer, who has been led to suggest that the luminous gas, which undoubtedly forms the visible portion of nebulæ, is gas volatilised from the solid state, and rendered incandescent by violent impacts between meteoric stones. These gases, he says, cool quickly, cease to be luminous and condense, but the collisions being incessant, the whole nebula shines with a steady light.

* See Phil. Trans. R.S. vol. 180A (1889), pp. 1–69.

It appears, then, to be probable that the immediately antecedent state of the sun and planets was not a continuous gas, but was a swarm of loose stones. Here, then, there arises a dilemma; for on the one hand the meteoric theory denies the continuity of the matter forming nebulæ, whilst on the other hand the nebular hypothesis demands such continuity.

The object of this lecture was to show that there is, however, a way in which these two apparently conflicting ideas may be reconciled.

In order to prepare the way for the suggested reconciliation, a sketch was then given of the Kinetic Theory of Gases, according to which a gas consists of a great number of elastic molecules, moving at high speed in all directions at hazard, and continually coming into collision with one another by chance.

According to this theory a pigmy, of a size comparable with the average distance between adjacent molecules, would be conscious of the blows he received from individual molecules, and he would have lost the sense of gaseous pressure, which arises from impacts too numerous and too rapid for discrimination. Thus, what is called gaseous pressure is a question of the magnitude of the observer.

The suggestion, then, of this lecture was that celestial nebulæ are of such large dimensions, that meteorites might be treated as molecules and that their collisions might impart to a nebula, as a whole, the quasi-gaseous mechanical properties demanded by the nebular hypothesis.

But if such a suggestion is to rise above the level of mere conjecture, it demands a careful examination in detail. It is accordingly necessary not only to consider the details of an individual collision, but also to examine whether a meteoric medium is of sufficiently fine texture to fulfil the conditions imputed to it.

The kinetic theory of gases requires that the molecules of gas should be perfectly elastic, and, although meteorites are certainly not perfectly elastic, it was maintained that the sudden volatilisation of gas, at the point of contact of two of them at the moment of collision, would act as a violent explosive between them, and would impart to them a virtual elasticity of considerable perfection.

The investigation of the degree of fineness of grain necessary to admit the applicability of the theory involved numerical calculation, and the requisite data had necessarily to be derived from the solar system.

If the sun's mass were broken up into iron meteorites, each weighing say a pound, the dimensions of each meteorite would be known, and their number would be four followed by thirty zeros.

These iron stones were then supposed to be distributed in a swarm extending beyond the present orbit of the planet Neptune. To give numerical precision, the swarm was taken to extend as far beyond Neptune as Saturn now is from the Sun.

These supposed conditions were adopted merely by way of an example which should represent a nebula of extreme tenuity; for if the meteorites were not too sparsely distributed to impart quasi-gaseous

properties to the whole in this supposed case, the nebula would *à fortiori* possess those properties when it had shrunk to smaller dimensions. The swarm was supposed to be arranged in a perfect sphere, and what may be described as the layers of equal density of population were taken to be concentric spheres, but the density of population would necessarily be much greater towards the middle than towards the outside.

The whole crowd of stones would arrange itself automatically into a steady condition, in which the population had no tendency to shift, although, of course, the dance and collisions between the constituents of the crowd would be incessant. When this steady condition was submitted to calculation, it was possible to discover the average velocity of the stones, the average density of population, and the average frequency of collision at each point of the swarm.

It will naturally occur to the reader to inquire as to the source of the great velocity of the stones; it arose from gravitation, the stones having fallen in from a great distance towards a centre of aggregation.

If somewhere in space there were an aggregation of meteorites, and if a stone were released from a state of rest at a very great distance, it would fall towards the swarm under the influence of gravitation. On reaching the swarm it would have acquired a certain velocity, and would penetrate to some uncertain distance, until it happened to strike another stone. Henceforth its path would be zigzag, as it happened to strike, and it became incorporated as a member or molecule of the swarm.

The supposed visitant from outside space imported energy of motion into the swarm, and besides increased the total mass of the swarm. Thus, if it be imagined that the swarm is increased by the addition of stone after stone, each being let fall from a distance, it is clear that, in the course of accretion, the energy of agitation of the meteorites continually increases. When stones have ceased to fall in, the materials of the nebula were collected, and by means of incessant collisions the swarm gradually attained the steady condition above referred to.

By reasoning of this kind it was possible to discover how fast the stones were moving, but it is proper to add that an important correction had to be applied to allow for the fact that at each collision between two stones some speed is lost. In the process of settling down into the steady condition, each stone loses, by imperfect elasticity, three-tenths of the speed it would have if it were a fresh arrival from space.

It makes no material difference in the result by whatever process the stones were collected together, and the account given above of the formation of a swarm was not intended as a contribution to its history, but was only meant to explain the mechanical principles involved.

By this line of argument it may be concluded that when the solar swarm extended half as far again as the planet Neptune, the average velocity of the stones was three miles a second.

It was next necessary to find out how often the stones came into collision, how far they travelled from one collision to the next, and whether the collisions could be frequent enough to impart to the whole nebula the gaseous property demanded by the nebular hypothesis.

Even a microscopic animal in our atmosphere is not aware of the individual impacts of molecules on his body, and his sensation is still that of gaseous pressure. But it must clearly be a giant who would not be aware of the individual blows of meteorites in a meteoric nebula, but would only realise their average effects.

It would not be easy to explain the exact reasoning by which it is possible to determine how large the giant must be in order to act as a judge of the gaseous property of the meteoric swarm, nor of how a comparison of his dimensions with the texture of a meteoric swarm is to be made, and it must suffice to say that the comparison is best clothed in a form which may appear something quite different, but which is really substantially the same.

It may be stated, then, that a meteoric nebula would behave sufficiently like a gas to allow the nebular hypothesis to be true, if the average path of a meteorite between two collisions were only a short portion of that curved orbit which it would describe under the action of gravitation if it could move through the swarm without ever colliding with another stone.

These explanations led on to the numerical values derivable from calculation, on the hypothesis that the solar nebula, consisting of 1 lb. iron stones, was distributed in a swarm extending half as far again as the present distance of the planet Neptune from the Sun.

It appeared, then, that at the middle of the swarm a meteorite would, on the average, come into collision every 13 hours, and would travel 140,000 miles between collisions; at the distance of the small planets called the asteroids, it would collide every 17 hours, and would travel 190,000 miles between; at the distance of Uranus the collisions would be at intervals of 25 days, and the path 6,000,000 miles; and lastly, at the distance of Neptune, the interval would be 190 days, and the path 28,000,000 miles.

It may also be shown that the path described between collisions forms a larger portion of the whole curved orbit of a meteorite the further we go from the middle of the swarm. Even at the distance of the planet Neptune the collisions were, relatively speaking, so frequent that, on the average, gravity only sufficed to draw the meteorite aside from the straight path by 1-66th of the length of path it had traversed, before it was deflected into a fresh orbit by collision with another stone. The fraction 1-66th was then the numerical value of the criterion of the applicability of quasi-gaseous properties to the swarm, and this fraction is so small that it may be concluded that the swarm passes the proposed test.

It followed, therefore, that if meteorites possess a virtual elasticity, a swarm of meteorites provides a gas-like medium of fine enough structure to satisfy the demands of the nebular hypothesis.

The result of this discussion then appeared to justify the opinion that the meteoric theory may be reconciled with Laplace's hypothesis, and that they may both be held to be true.

After this discussion of the proposed modification of Laplace's hypothesis, it was natural to turn to the series of events which may be supposed to have occurred after the nebular stage of evolution.

At the various centres of condensation, which now form the sun, planets, and satellites, the swarm of meteorites must be supposed to have become denser, and the collisions too frequent to allow the gases to condense again, so that by degrees all solid matter in the neighbourhood of such centres would be volatilised. Away from these condensations there were still many free meteorites, but the majority of those which formed the swarm in primitive times would have been absorbed, and the absorption would still go on gradually.

The collisions amongst the free meteorites became rarer and less violent, and finally, when relative motion was nearly annulled, almost ceased to occur. The residue of the meteoric swarm then consisted of sparse flights of meteorites moving in streams. There is evidence of the existence of such streams at the present day in the zodiacal light, in falling stars, and in comets. But these are the dregs and sawdust of the solar system, and merely give a memento of the myriads which must have existed in early times, before the sun and planets were formed.

The subject of this lecture is a large one, and the limits of time rendered it impossible to do more than speak of the more prominent features of the problem. The value of the investigation of which some account has been given, will appear very different to different minds. To some it will stand condemned as altogether too speculative; others may think that it is better to risk error on the chance of winning truth. It was, however, contended by the lecturer that the line of thought flowed in the channel of truth, and that by its aid many other interesting problems might perhaps be solved with sufficient completeness to throw further light on the evolution of nebulæ and of planetary systems.

[G. H. D.]

WEEKLY EVENING MEETING,

Friday, February 1, 1889.

John Rae, M.D. LL.D. F.R.S. Vice-President, in the Chair.

Professor W. C. McIntosh, M.D. LL.D. F.R.S. &c.

The Life-history of a Marine Food-fish.

It is but a few years since the life-history of our most important marine food-fishes was involved in considerable obscurity—not only as regards popular views, but even in respect to the knowledge of men of science. Thus, for instance, in the years 1883 and 1884 the almost unanimous opinion of British fishermen was that our common food-fishes sought the shallow water of the bays and inshore ground generally for the purpose of depositing their eggs on the bottom. No observations specially bearing on this point had been made by British zoologists, and a series had to be undertaken for a public inquiry then in progress—with a result which demonstrated how extensive the reverse of the popular notion was. Again certain comparatively recent authors on British fishes speak of a common fish like the gurnard as spawning twice a year, whereas, after careful observation, no evidence in support of this view has been obtained. The same obscurity veiled the larval and post-larval conditions of most of the food-fishes, even G. O. Sars—in regard to the latter stage—describing no intermediate forms between the larva of 6 mm. and the post-larval stage of 24 mm. in the cod—almost the only fish to which some attention had been paid.

On the other hand, our knowledge of the development and life-history of the fresh-water fishes—such as the salmon, trout, and charr—has for many years been well understood—thanks to the labours of Louis Agassiz and Vogt in Switzerland, Coste and Lereboullet in France, Ransom in England, and Shaw in Scotland, on the scientific side, and of the noblemen and gentlemen of Perthshire (ably seconded by Robert Buist) in connection with Stormontfield Ponds on the Tay, on the popular side. Much information has also been recently obtained by Dr. Day and Sir J. Gibson Maitland at the excellent ponds of the latter at Howieton.

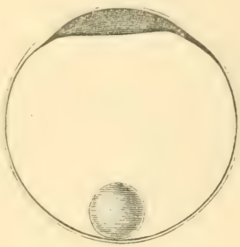
A short time ago, relying on experience derived from fresh-water fishes, not a few imagined the eggs of marine fishes as readily visible and tangible objects—possibly associated in their minds with certain practices in trout-fishing, or it may be with the manufacture of caviare. Recent investigations, however, have shown that in most

marine food-fishes the eggs are minute glassy spheres which float freely in the ocean. For a knowledge of this fact we are indebted in the first instance to Prof. G. O. Sars, of Christiania, a naturalist trained from boyhood under a distinguished father, and who by a fortunate appointment to a fishery post in Norway, was enabled to discover that the eggs of the cod, haddock, and gurnard, floated in the water, or, as we term it, were pelagic. He thus opened up a new field in the economy of the food-fishes, which in a great maritime country like ours ought not to have remained so long unexplored.

Lately, however, attention has been earnestly directed to the subject, and the labours of Cunningham, Brook, Prince, and others have made considerable advances in this department.

It is now known that the great majority of our British marine food-fishes—indeed all our most valuable kinds (including even the sprat and the pilchard amongst the clupeoids) produce minute eggs (Fig. 1)—as transparent as crystal, and which float freely throughout the water. These eggs, moreover, are not all shed at once, as in the case of the salmon, but successive portions of the ovary become ripe, and the eggs then issue externally. If by any accident or irregularity—as for instance the confinement of a flounder in an unhealthy tank—this gradational issue is interfered with, the animal dies from the great distention caused by the pent-up eggs. In the case of the cod this gradual issue of the eggs continues probably for a week or two, so that the progeny of a single fish in one season may vary considerably in size.

Fig. 1.*



Pelagic egg of the Ling, enlarged.

From the early months of the year onward to late autumn the sea off our shores thus abounds with pelagic eggs, those of the rockling, haddock, and sprat being amongst the earlier forms, while the later include those of the sole. As indicated in the Trawling Report, and now supported by further experience, it would be a very difficult matter indeed to arrange for a close time in the sea, that is to say, for a limited period during which the mature fishes might be permitted to spawn in peace. This, however, in the case of individual species, such as the cod, might more readily be carried out, so as to save the mature fishes at the spawning period.

In a vessel of still sea water these transparent glassy spheres rise at once on issuing from the fish, and form a stratum on the surface. Even the ripe portions of ovaries removed from the rejected viscera on a pier will show the same features, and thus, indeed, they first came before the lamented Lord Dalhousie at

* I am indebted to Mr. E. E. Prince, B.A. for kindly aiding me with sketches for the woodcuts. The sketch of *Motella* is by Dr. Scharff. When floating freely in the sea the blastodisk is inferior.

Anstruther. In the sea, however, they are seldom met with on the surface, and the tow-nets require to be sunk a fathom or two to capture them, for their specific gravity is so little less than that of sea water that they are carried hither and thither by the currents in every direction. Some indeed are captured near the bottom by nets attached to the trawl-beam, while experience with the large net of the St. Andrews Laboratory has proved that a great number are carried in mid water.

When these glassy eggs issue from the female fish they are soon fertilised in the surrounding water: so that in British seas at any rate non-fertilisation is one of the rarest conditions in pelagic eggs. It is indeed more likely to happen in the case of the herring, which deposits its eggs in masses on the bottom, or in artificial circumstances in tanks. The unfertilised egg soon becomes opaque and sinks, so that it is readily recognised.

In this connection I would again refer to the notion not long ago firmly rooted in the minds of many—especially those practically engaged in fishing—that the fishes at the spawning season seek the shallow water in which to deposit their eggs. Now there is little in nature to support this idea. Shore-fishes it is true, such as the lump-sucker and sea-scorpion (*Cottus scorpius*), do deposit their eggs there (and there cannot be a doubt that some of the masses of eggs thus deposited have been mistaken for those of the food-fishes), but the edible fishes proper, such as the cod, haddock, whiting, flounders, and others, appear to produce their eggs just where they happen to be feeding at the season. Their eggs are taken in charge by the ocean generally, and hence are independent of any imaginary protection or privilege pertaining to the shallow waters.

Moreover, it does not follow that the fishes of an enclosed bay * will increase of themselves. As in the case of the plaice, in shallow sandy bays, it may happen that most of the large mature fishes are beyond the limits, the half-grown or immature forms mainly occurring within; pelagic ova therefore must be borne inward, and still more the pelagic young, while the post-larval stages likewise migrate shorewards; a counter-migration of the older forms subsequently taking place to the deeper water. Such bays therefore, have to depend for their stock of fishes on the unprotected offshore. If by any chance the latter waters were depopulated the inshore would seriously suffer.†

The minute size of the eggs of all the important marine food-fishes enables a fish like the cod, for instance, to produce an enormous number—probably about 9,000,000 as against the 18–25,000 of the salmon, or the 10–30,000 of the herring, both of which fishes deposit

* For example, closed by a Fishery Order.

† This feature was pointed out in the 'Report of H.M. Trawling Commission, under Lord Dalhousie.'

their eggs on the bottom. In the same way the very small eggs of the dab provide for a large annual increase of the species.

The translucent eggs, which, unless they contain a globule of oil, as in Fig. 1, are difficult to see in some instances even in a glass vessel, thus escape (by floating throughout the water) the vicissitudes to which a purely surface-life would expose them, such as the admixture of the surface-water with rain, and the attacks of gulls, ducks, and other forms; and they also are less at the mercy of the active predatory races living on the bottom, not to allude to the risks of being swept by storms on the beach or captured and destroyed by the ground-rope of the trawler. Nature indeed could have devised no method more secure than this for the safe increase of those valuable fishes which for ages have peopled our waters, and, with Prof. Huxley, I venture to say, will perhaps people them for ages yet to come, notwithstanding the persistent efforts of man to annihilate them.

Some good observers, for example, Prof. Ryder in America, have attached much importance to the oil-globule in eggs which are pelagic; but its buoyant influence has been slightly over-estimated, for, as he himself shows, some contain no oil-globule, while the massive oil-globules in the eggs of the salmon and cat-fish have no such effect.* They float, as well shown by Mr. Edward E. Prince,† solely in virtue of their specific gravity, which is somewhat less than that of sea-water. The moment fresh water is added they sink, as they likewise often do when transferred from a vessel filled at sea into one containing shore-water.

While immediately after deposition these minute spheres are prone to accident from impurity and sudden changes in the temperature of the water, such would not seem to be the case after development has made some progress. Thus many living eggs will be found in odoriferous vessels brought from sea by the fishermen if the enclosed embryos have reached an advanced stage. Again, while carrying out some experiments on temperature (at the suggestion of Prof. Huxley) during the Trawling Expeditions, I had occasion to heat a test-tube containing some of the eggs of the flounder, so as to make them rush up and down the vessel most actively. Considerable heat was applied, and under the impression the eggs were irretrievably

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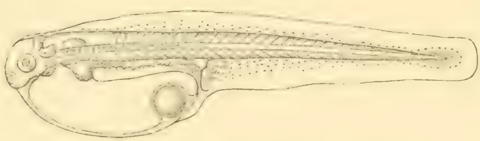
* Prof. Ryder classifies buoyant ova into (1) those in which the specific gravity of the yolk is diminished, ex. cod; (2) those in which large oil-drops in an eccentric position aid in causing the eggs to float; and (3) those in which a very large oil-drop caused the ovum to float even in fresh water. Mr. Prince has shown that the number of buoyant eggs without oil is at least equal to the number with oil-drops. The statement of the former, therefore, that the cod's egg is unique in floating by the diminished specific gravity of the protoplasmic matter of the vitellus does not traverse the view of the latter. Moreover, it must be remembered that the oil-globule in many is not permanently eccentric, but moves throughout the yolk.

† Secretary to the Mussel and Bait Committee.

injured, the tube was set aside. Some days afterwards, when explaining the nature of the experiment to Prof. Ewart, he noticed motion in the tube, and further examination showed that after all this exposure to heat the little flounders had emerged as usual, and were alternately floating and swimming about in the water. On the other hand, severe frosts are fatal to ova crowded in shallow vessels, in many cases actual rupture taking place ;* and the same occurs in large eggs, for example those of the catfish, deposited on the bottom of the vessel.

Out of the little glassy sphere, after a longer or shorter interval (varying from a few days to a few weeks, according to temperature), comes a minute and nearly transparent fish (Fig. 2) which at first is often as passive in the currents as the eggs themselves.† It soon, however, uses its tail for swimming and its pectoral fins for balancing. Its shape is somewhat like that of a tadpole, partly from the large head, but mainly from the great size of the yolk-sac, which contains a store of nourishment on which the little mouthless creature, about

Fig. 2.



Larval Ling, immediately after hatching.

3 mm. long, sustains itself for a week or ten days. In this respect it somewhat resembles the young salmon in which a much larger collection of the same food supports it about six weeks amongst the gravel in the spawning-bed of the river, though a closer scrutiny reveals certain essential differences. Thus the store of nourishment in the yolk-sac of the salmon is taken up by the blood-vessels which branch in a complex manner over the whole yolk, whereas in the young cod, though the heart is present and pulsating, not a blood-vessel at first is seen, and none ever enters the yolk-sac. The absorption of this nourishment therefore must take place by aid of the cells and tissues themselves, and there is nothing specially wonderful in this, when the conditions in the endoderm of *Hydra*, and other instances of intracellular digestion are considered.

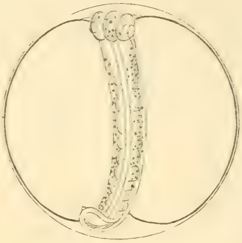
It has been mentioned that these minute and most delicate little

* 'Nature,' June, 1886.

† For some years the development of fishes has been studied by able workers, amongst others on the Continent, by Götte, Kupffer, Hoffman, Henneguy, E. Van Beneden, Owjannikov, and Rafaele; in America, by Alex. Agassiz, Ryder, and Whitman; while in our own country, Ransom, Klein, Cunningham, Prince, and Brook have carried out similar researches.

fishes are nearly transparent, and this is more or less the case throughout, though in the majority—even before they leave the egg—points of pigment appear here and there in the skin, so as to give them a distinctive character (Fig. 3). After hatching, these pigment-spots branch out in a stellate manner, thus becoming more evident, and it is found that in most cases each little food-fish has colours of its own. Thus the cod (Fig. 4) is known by its four somewhat regular black bands, the pigment on the haddock being less defined, the whiting by its canary-yellowish hue, the gurnard by its chrome-yellow, the ling by its gamboge-yellow, the flounder by its yellow and black, and so on. All these hues, however, become greatly modified during subsequent development, indeed the pigment in no group of vertebrates shows more remarkable changes between the young and adult states than certain of our food-fishes. Thus for instance the cod is characteristically speckled in its tiny youth

FIG. 3.



Flounder, showing pigment in the egg.

FIG. 4.

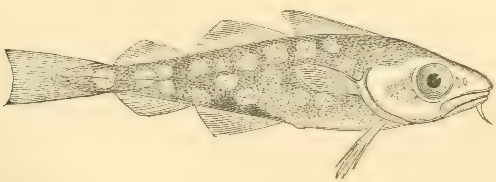
Larval Cod with black spots or bands, slightly enlarged.

FIG. 5.

Aggregation of pigment in Post-larval Cod.

(Fig. 4), next it becomes more or less uniformly tinted, then the pigment groups itself somewhat irregularly on the sides (Fig. 5); thereafter it is boldly tesselated (Fig. 6), subsequently blotched

FIG. 6.

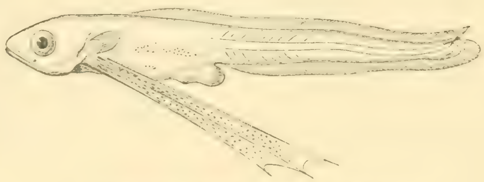


Tesselated condition of young Cod (spirit-preparation).

with reddish brown, and finally in its adult condition it again puts on more or less uniform tints. The ling shows a similar series

of transformations, the colours, however, differing in their arrangement, being marked with gamboge-yellow in its larval, slightly barred in its early post-larval stage, then the body becomes more or less uniformly tinted in its post-larval phase, and the little fish is furnished with a pair of enormously developed and bright yellow ventral fins (Fig. 7)—so different from the short ones of the adult.

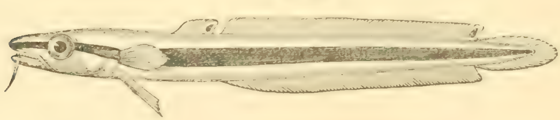
Fig. 7.



Long-finned Post-larval Ling, enlarged.

It is next striped longitudinally when about three inches long (Fig. 8), thus affording a great contrast to the tesselated condition of the young cod. In this stage an olive-brown band passes from the tip of the snout in a line with the middle of the eye, straight backward to the base of the caudal fin-rays. The pale ventral surface bounds it inferiorly, while a dorsal stripe with a beautiful opaline lustre runs from the tip of the snout, over the upper part of each eye to the tail, on which it is opaque white, thus giving the

Fig. 8.



Young Ling, about 3 inches long (in spirit).

fish a characteristic appearance. The dorsal line from the brain backward is distinguished by a narrow edge of dull orange or pale olive, which relieves the colours formerly mentioned, and the general effect is varied by two black specks in the dorsals. When it is double the length (i.e. 6 or 7 inches) a complete change has taken place in its coloration (Fig. 9). Instead of being striped the fish is now boldly and irregularly blotched—both dorsally and laterally, the region of the white stripe being indicated by the pale and somewhat scalloped area dividing the dorsal from the lateral blotches. Fourteen or fifteen brownish blotches occur between the pectorals and the base of the tail, and they are separated by the

whitish areas, which thus assume a reticulated appearance, and both kinds of pigment invade the dorsal fins. Other touches of dark

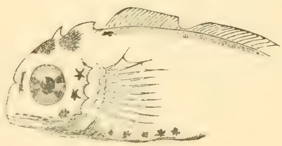
FIG. 9

Young Ling in the barred stage. About half natural size.

pigment on the fins and tail increase the complexity of the coloration at this stage.

Again, some species, like the gurnard, have pigment over the yolk-sac before they are hatched, others have not. The dragonet in its post-larval (and pelagic) stage has its ventral surface deeply tinted with black pigment, while in the adult, a ground-loving fish, it is white. The St. Andrew's cross in the eye of the post-larval four-horned Cottus (*C. quadricornis*) is another remarkable feature (Fig. 10). No more interesting or more novel field, indeed, than this exists in the whole range of zoology; but the investigations need ships and boats, with expensive appliances, as well as persevering work for several seasons. We have only been able to open the field at St. Andrews by the help of the Trawling Commission under Lord Dalhousie, and subsequently by the aid of the Fishery Board. It may be asked, Why is all this remarkable variation in colour? Just for the same reason that the young tapirs and wild pigs are striped, or the young red deer spotted—the adults in each case being uniformly tinted. Such features indicate their genetic relation with ancestral forms having these marks; and, moreover, in the struggle for existence, such variations in tint conduce to the safety of the young.

FIG. 10.



Head of *Cottus quadricorni* with St. Andrew's cross in eyes.

The view of Eimer that the markings in animals are primitively longitudinal would not suit for many fishes, notably for the young cod, ling, and Pleuronectids, and, indeed, Haacke has already pointed this out from a study of the Australian fish, *Helotes scotus*,* the adult of which is marked by eight longitudinal bands, while

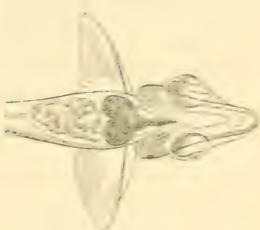
* One of the Pristipomatidæ.

young specimens present in addition a row of clear transverse bands which subsequently disappear.

The larval salmon enters the world of a size—though small—that is readily recognisable, viz. about three-fourths of an inch in length, but the marine forms under consideration, from their minute size and glassy translucency, are almost invisible to the naked eye—just a gleam of light broken by the passage of a different medium, or a tinge of pigment, arresting attention. Only in the cat-fish (which is not much—though it ought to be more—of a food-fish) with its large egg, have we a size nearly reaching that of the salmon at birth.

We had left the larval fish tossed about by the currents and unable to struggle against them, now floating with its yolk-sac uppermost, or hanging in the water with its head downward, and again making spasmodic darts hither and thither. Soon, however, it gathers strength, and at the end of a week or ten days it glides actively through the water, and avoids both obstacles and enemies, the young cod nimbly escaping the forceps, poising itself in the water with its large pectoral fins (Fig. 11), and evincing both intelligence and dexterity. Moreover, this activity greatly promotes respiration in those like the gurnard with a motionless mandible, the water being thus sent through the mouth and over the branchial region. Its mouth has now opened and the yolk-sac has been absorbed, while it feeds on the most minute of the little Copepods, especially those almost microscopic in size, that swarm in the surrounding water. The provision whereby such little fishes find in the ocean food suited to their capacities is one of the most striking features in nature, but it has only recently been carefully investigated.* It is a notion no longer tenable that during the winter and spring the sea—to a large extent—is devoid of the wealth of pelagic life so characteristic of the summer months—just as it is of the genial waters of the tropics. For several years, however, it has been known that a vast abundance of minute life of all kinds is present throughout the entire year—and from the surface to the bottom. Moreover, during the warmer months a constant succession of young forms

FIG. 11.



Anterior (ventral) region of larval Cod with great pectoral fins, magnified.

* *Vide* La faune pélagique du Golfe de Marseille, par Gourret. Ann. du Musée d'hist. nat. de Marseille, II. 1884. The pelagic fauna of our shores in relation to the nourishment of the young food-fishes. Ann. Nat. Hist. Feb. 1887. Also Hensen and Möbius in Fünfter Bericht der Kommission zur wiss. &c. der deutschen Meere. Berlin 1887, pp. 1 and 109.

rises from the eggs both of the sedentary and creeping animals on the bottom to the surface—where they sport in the summer sun, undergo certain changes, and again descend as they assume the form of the adult. The pelagic young food-fishes—swimming freely in the ocean—thus have a double chance at them—first in their very early stage as they rise, and again in their larger and later condition as they descend. The enormous numbers, countless variety, and ever-changing nature of the small animals either directly or indirectly constituting the food of these little fishes form an important feature in the economy of the sea. Such animal forms comprise those long known in the British seas, besides others more familiar to arctic voyagers, or to the sunny waters of the Mediterranean, for, with modern apparatus and persistent effort (thanks to the enlightened views of the Government acting through the Fishery Board), our knowledge is always extending.

It is a remarkable fact that it is primarily to plants in inshore waters that the abundance and variety of animals are in many respects due, especially if estuaries also debouch in the neighbourhood. Thus nowhere are the swarms of Sagittæ, Appendicularians, Crustaceans, and other forms of fish-food more conspicuous than in the midst of a sea teeming with diatoms, Rhizosoleniæ, and other algoid structures.* These nourish many of the lower forms upon which the crustaceans and other higher types feed, the latter again falling a prey to the fishes. Moreover, while the larger forms of the Copepods and other crustaceans, for example, afford suitable nourishment for the more advanced post-larval fishes, the multitudes of larval crustaceans (*Nauplii*) are adapted to the needs of the smallest larval food-fishes. Now this plant-life is specially abundant in April and May, just when the larval and very young post-larval fishes appear more abundantly in the inshore waters, so that the cycle is nearly complete, viz. from the inorganic medium—through microscopic plant and larval crustacean—to the post-larval fish. I have mentioned the neighbourhood of an estuary as a prolific source of food for young fishes, and I need only explain further by instancing the case of mussel-beds, which for months pour countless myriads of larval mussels into the adjoining sea, far beyond the needs of the area as regards mussel-culture, and which form a favourite food of the little fishes at all stages, but especially from an inch and a half to three inches in length. These fishes feed on the young mussels as they settle down on the sea-weeds, rocks, and zoophytes in August, after a free-swimming larval existence. Like some of the forms indicated above, mussels live to a considerable extent on microscopic plants and various minute organisms contained in the mud of the estuaries and other sites, so that a rich and favourite food, universally liked by fishes, is the product of these uninviting flats. Moreover, in passing, it may be remarked that, while everywhere preyed on by the food-fishes, it

* The fact that certain fishes feed on Infusoria has not been overlooked.

occasionally happens that in turn the mussel proves a source of inconvenience to them, for, settling on the gill-arches of haddocks, the mussels flourish on a site so suitable for aeration and food that they by-and-by press out the gill-cover and impede respiration, just as the shore-crab (which is also fond of mussels) has its eye-stalks wrenched out by the slow but sure growth of the young mussels which have fixed themselves in their sockets. Nemesis thus, by a chance of anchorage, converts a favourite food into a permanent inconvenience.

Again, in connection with the pelagic food of fishes, it is a well-known fact that adult cod are extremely fond of sea-anemones,* and some of the rarest species may be procured in their stomachs, a feature by no means surprising when we remember that Abbé Dicquemare cooked and ate his sea-anemones with great relish, and wrote in their favour, as also did Mr. Gosse in our own country. Now, the pelagic young fishes, instead of roaming near the bottom in proximity to the anemones fixed on the rocks, and running the risk of being themselves captured for food, find in the inshore waters in summer the larval *Peachiæ* in great numbers conveniently attached by the mouth to the little hydromedusæ (*Thaumantias hemisphærica*, and *T. melanops*) which occur in swarms in mid-water. Moreover, the somewhat larger young food-fishes (2–3 inches) show the same liking for the cœlenterate group, by browsing on the zoophytes (*Obelia geniculata*) which cover the stones and rocks with feathery tufts, yet the zoophytes are not much the worse for this treatment, for they by-and-by shoot afresh, and clothe the area once more with a minute forest. The rapidity with which such zoophytes grow is remarkable, though we must remember that in some cases the old stock naturally dies off after having produced swarms of pelagic young.

Under this rich food, the young fishes grow apace—head and eyes, mouth and accessory organs, body and fins—all rapidly increase, and the little fish, hatched in the spring, say from March to May, is soon in what is known as the post-larval stage, that is, has lost its yolk-sac, has assumed a more or less uniform tint, and has gill-fringes and teeth. It is about a quarter of an inch long, and is both active and intelligent, the large head and large eyes of the young food-fishes being at this stage specially conspicuous, and in marked contrast with such as *Cottus*. The marginal fin is quite continuous at a quarter of an inch, and the lancet-like termination of the caudal end of the body is noteworthy.

About this time the ventral fins of the pelagic fishes first make their appearance, for hitherto they have managed to do without them. Moreover, these fins in some, such as the rockling and ling, undergo remarkable development, forming in the latter (Fig. 7) a pair of great ventral wings conspicuously coloured yellow; yet in the adult (a

* A favourite bait for cod in some parts (e. g. Aberdeen and St. Andrews), and from the fact, amongst others, that star-fishes do not molest them on the hooks, no bait is more successful.

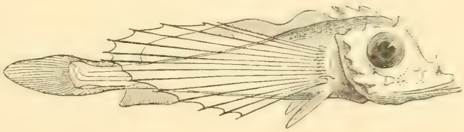
ground-fish) they attain no greater dimensions than in the cod, both having at a certain stage soft, free filaments or tactile processes at the tip. The ventral fins in the post-larval rockling are equally large, the distal half being black, so that at first sight the little fish when captured seems to possess a great ventral spine on each side (Fig. 12).

FIG. 12.

Post-larval Rockling, enlarged.

In the post-larval gurnard, again, the huge pectoral fins form a drapery for the entire body when folded back, only the tip of the tail extending beyond them (Fig. 13). They are indeed proportionally as large as in the southern flying gurnards, but in these the fins reach full development only in adult life, while in the young stages they are comparatively small—exactly the reverse happening in the grey gurnard of our seas. The presence of the broad arches of

FIG. 13.



Post-larval Gurnard, enlarged.

pigment on the pectorals of several forms, such as the present species, green cod, and armed bullhead, is also an interesting feature. We have not yet read the riddle of all these changes, but in the ling the great ventral fins are probably connected with its roaming or pelagic life, and this explanation would also suit in the case of the rockling, both in their mature state seeking their food on the ground.

The little fishes at this stage are still more or less translucent, except in the region of the eyes, which are silvery, and on the parts where the pigment occurs. Moreover, their fondness for a minute reddish Copepod (*Calanus finmarchicus*), which occurs in myriads around them, gives the region of the stomach a faint pinkish hue from the translucency of the tissues. By-and-by, however, pigment appears, foreshadowing in the cod (Fig. 5) those peculiar squares which give the sides, at a somewhat later stage, their tessellated or tartan-like aspect. Besides, they are found nearer the bottom of the water, so that they can be captured in a naturalist's trawl with a fine gauze bag at the end. There is, therefore, a downward tendency as the little fishes get older

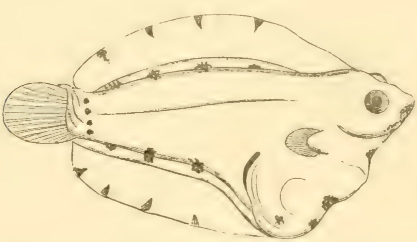
and stronger, and thus in many cases a parallelism exists between them and the minute forms on which they prey, for the eggs rise on deposition towards the surface, where the helpless larvæ (or newly hatched young fishes) also often occur, and then they seek the lower regions of the water as their size increases.

There is much that is wonderful in such a life-history, especially in the metamorphoses or changes of form undergone by many of our best fishes such as the flat fishes (Pleuronectidæ), which come out of the egg just like a haddock or cod, with an eye on each side, as in Fig. 2, yet in after life have both eyes on the same side. Nothing like this occurs in any of the higher vertebrates. Gradually during growth the body of the fish increases in depth (Fig. 14), then the right or left eye passes over (Fig. 15) the ridge of the head to the opposite side, while the creature, hitherto pelagic, sinks deeper in the water and exhibits a tendency to lie on the side from which the eye has passed, and which gradually loses its dark pigment so as to become white.* It finally reaches the bottom, taking up its residence amongst the sand or sandy mud, and lying with the two eyes and the coloured side up, the white underneath. The mode by which the eye travels round has been a fruitful source of discussion with scientific men, and

FIG. 14.

Young "Witch" (*Pleuronectes cynoglossus*) in the third stage, enlarged.

FIG. 15.



Young "Witch" at a later stage, the left eye just appearing on the ridge of the head, enlarged.

amongst these the names of Steenstrup, Malm, Schiödte, and Alex. Agassiz abroad, with Wyville Thomson and especially Traquair in our own country, are well known. The fact is—two methods exist in nature; in the one the eye travels over the ridge of the head, as just

* The tardy disappearance of the pigment in some forms is interesting.

described in the flounder; in the other it traverses the soft and yielding tissues of the young fish, and so gains the other side. In *Plagusia*, the species in which the latter remarkable change occurs in the post-larval stage, the general tissues are so transparent that the creature in a glass vessel can only be noticed by the two apparently disembodied eyes, or by the gleam of light caused by its movements; and before the change ensues in its eyes it can look obliquely through its own body and see what passes on the other side.*

Up to this stage in the life-history of both round and flat fishes it will have been apparent that the efforts of man can have little effect on the vast multitudes of the eggs and minute fishes. His trawl sweeps beneath them, or they are carried harmlessly through its meshes. Not even in the case of a trawl blocked by a fish-basket and several large skate are any likely to occur. No example indeed was procured in the trawling expeditions for the Commission under Lord Dalhousie. The hooks of the liners are too large for the mouth at this stage, and hence they escape capture. Their small size and translucency also seem to afford protection in the case of predatory fishes of their own or other kinds, for they are rare, so far as present observation goes, in the stomach of any fish. Their great numbers are doubtless kept in check by some means, and we know that even jelly-fishes (e. g. *Pleurobrachiæ*) are very fond of post-larval fishes. It is only when they become somewhaf larger that they are preyed on by their own and other species, and are swept up in thousands by the destructive shrimp-nets on our sandy shores.

While the little food-fishes are assuming the change of hue indicated in the preceding pages, they in many cases seek the inshore waters; at least systematic use of the mid-water and other nets prove that at certain seasons they are met with in large numbers at the entrance to bays or off shore, and that a little later—in the case of the cod from the first of June onwards—they are visible from the rocky margins. The coloration in this species (cod) is now beautifully tessellated (Fig. 6), and they swim in groups, often in company with the young green cod, at the margin of the rocks at low water, and in the little tidal bays connected with rock-pools. The latter are often richly clothed with tangles, bladder-weed, red and green seaweeds, and the green *Ulva*—amidst the mazes of which the young fishes find both food and shelter, capturing the little crustaceans (Copepods, Ostracods, and others) swimming there, and snatching the young mussels and minute univalve mollusks from the blades of the seaweeds. To the zoologist few sights are more interesting than to watch the little cod in these fairy lakes, as they swim in shoals against the current, balancing themselves gracefully in the various eddies by aid of their pectoral fins. In a mixed company, the young cod are easily recognised by their coloration, and the reddish hue of

* Alex. Agassiz, 'Proceed. Americ. Acad. Arts. and Sc., vol. xiv. p. 8, 1878.

the occiput, for the blood-vessels there shine through the tissues, which generally are more translucent than in the green cod.

Prof. G. O. Sars considered that about this stage there was an intimate connection between them and the hordes of medusæ (*Aurelia* and *Cyanea*) which abound in the inshore waters towards the end of summer. He thought the young cod approached the medusa for the sake of the minute pelagic animals stupefied by its poisonous threads, and that the fish repaid this favour by picking off a parasitic crustacean (*Hyperia medusarum*) which clings to the medusa. Observations, continued for a long period in this country, however, show that this connection is only casual and of very little importance, and that certain *Hyperiæ* are occasionally found in vast numbers in a free condition.

As the season advances, the young cod are joined off the rocky ledges by a few pollack and whiting, but not by the haddock, which appears to have certain social views of its own—keeping probably a little farther out. The size of these cod late in autumn, as in October, varies, some reaching 4 to 5 inches in length. Their food ranges from zoophytes to crustaceans, mollusks, and small fishes, and in confinement the larger are voracious, an example about 5 inches readily attacking a smaller (3 inches) and swallowing it as far as possible, though for some time a considerable portion of the body and tail of the prey projected from the mouth. Moreover, the tessellated condition becomes less marked, and as they approach 8 inches in length a tendency in some to uniformity of tint is noticeable. Many of those, however, that continue to haunt the rocky shores and the tangle-forests beyond low water still retain for some time mottled sides, and they are known by the name of rock-cod. Further, while their growth in the earlier stages is less marked, it is now very rapid—even in confinement. The exact rate of growth in the free condition in the sea is difficult to estimate, but the little cod of an inch and a half to an inch and three-quarters in June reach lengths varying from 3 to 5 inches in autumn, and in the tanks of the laboratory, specimens 5 inches in August attain 8 inches the following March. At Arendal, in Norway, where opportunities for watching the growth of cod in confinement have been supplied with a liberality yet foreign to our country, Dannevig found that the cod of 3 mm. in April reached only 15 mm. in June, a length somewhat at variance with the condition, as above stated, on our shores. In July they measured 2 inches, in September 3 inches and a half, and in October about $4\frac{1}{2}$ inches. The second year they attained 14 to 16 inches in length. In artificial circumstances, as well as in nature, it is found that great variation exists in the sizes of the young fishes of the same age, and this variation would not seem to be related to temperature.

At the stages just mentioned they now come under the notice of both liner and trawler, for young cod 5 or 6 inches in length occasionally take a haddock-hook, and those somewhat larger (9 to 18 inches) occur in certain hauls of the trawl, especially off a

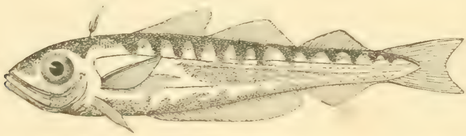
rocky coast like that of Aberdeenshire, south of Girdleness, as well as on the hooks of the liners on rough ground. Special trips, indeed were, and perhaps are, made by the liners for the capture of these young cod (termed codling), and thus their numbers are kept in check.

So far as present observations go, therefore, the young cod in a free condition reach the length of from 4 to 10 inches the first year, while in the second they attain from 10 to 20 inches or more. It probably takes 3 or 4 years (and this is the original opinion of Sars) or more to reach full maturity and a length of 3 feet or upwards; though he mentions having seen young cod a foot in length, with mature roe and milt, in the fish-market of Christiania. These, however, were probably abnormal examples.

Let us now glance at the condition in the whiting. Its earlier post-larval stages immediately following those observed in the tanks at the laboratory (for we failed to rear them) are even now somewhat obscure, but they probably approach those of allied forms, such as the cod and haddock. The characteristic nature of the larval pigment, however, would lead to the belief that perhaps in the brighter tints (e. g. yellow), differences may occur. Such, however, are lost before they come under observation; for all these delicate and minute forms are dead before reaching the deck, and indeed considerably altered. The pressure to which they are subjected in the large mid-water net by the crowds of hydromedusæ, and ctenophores alone would suffice for this, and the handling of the heavily laden net increases the dangers to forms so fragile. One about 12 mm. shows in spirit the dorsal and anal fins outlined though not separated from each other, and permanent rays occur in them and in the caudal. Minute ventrals are present, while the pectorals form large mobile fans. Groups of black pigment-corpuscles are distributed along the base of the dorsal and anal fins and over the brain, and a similar series occurs along the ventral median line of the abdomen. The sides have these blackish pigment-corpuscles more generally distributed than in the cod. No barbel is noticeable. When a little longer (15 mm.), the species is distinguished from the young cod by a more abundant distribution of black pigment-specks along the sides of the body and on the fins, and by the greater length and diminished depth of the first anal fin. The median line of pigment still runs along the ventral surface of the abdomen. At 20 mm. the characters that distinguish it from the cod of the same size are better marked, viz., the distribution of dense blackish pigment along the base of the dorsal fins; and it soon spreads downward over the sides. The first anal fin assumes the character of the adult, and a minute papilla indicates a barbel. Between the stage just mentioned and a length of 28 mm. a decided change in the dense dorsal pigment takes place, viz., a tendency to form separate groups or touches (Fig. 16). These differ from the cod in being confined to the dorsal region, though a few bars occur at the base of the tail. The fish is also now minutely flecked, all over the

head, sides, snout, and fins, with black pigment, and its general outline approaches that of the adult. It is at once distinguished from the young cod by the shortness of the snout, irrespective of the features

Fig. 16.



Young Whiting with serrated dorsal pigment-band, and parasitic *Chalimus*.

already pointed out, by the coloration, and by the shape of the first anal fin.

The differentiation of the two species, viz. the cod and the whiting, is very marked in spirit at the length of 34 mm. In the whiting the median dorsal fin is less abruptly elevated than in the cod, and the first anals diverge widely, the elongation of the latter being probably connected with the abbreviation of the abdomen. The body of the whiting is more plump and neatly rounded than in the cod, which is flatter and has generally a more prominent abdomen. The pigment-specks closely cover the sides of the body in the whiting, as well as the membranous webs of the dorsal fins, and are continued on the head. The pigment at the base of the caudal rays is more distinct in the whiting, and the lancet-like caudal termination of the body is longer in this species. The myotomes are coarser in the cod, and the surface has little of the dappled silvery sheen of the whiting. The chromatophores are larger in the cod, and are grouped in blotches over the surface, with intermediate pale patches, and the shoulder and head have much less pigment than in the whiting. Both the pectoral and ventral fins of the cod are shorter than those of the whiting. The snout in the latter is shorter and broader as well as deeper, and the short sub-mental papilla is in contrast with the long barbel of the cod of the same length. The whiting, produced from an egg of larger size, would appear to attain a plump body and finished outline sooner than the cod.

The foregoing stages are very abundant in autumn in the deep water off the Isle of May and the mouth of the Forth, but they also appear west of Inchkeith in the latter estuary. They are indeed more characteristic of the former region, as far as present observations go, than of the shallow water of the open bays such as St. Andrews, though on reaching a somewhat larger size they are quite common in the latter expanse. Both they and the cod in these early stages are infested by a crustacean parasite (*Chalimus*), which adheres to various parts of the head and body, just as the larval *Anceus* tenaciously attacks the young flounders in tidal harbours and inshore grounds.

The young whiting at a later stage (3–5 in.) joins the young cod at the margins of the rocks, and forms independent shoals in tidal harbours, as well as occurs some distance off shore, being frequently got in the mid-water net in the deeper water. Towards the latter size (6–7 in.) it readily takes the hooks of the liner, and in certain bays the multitudes of young whiting prove an inconvenience to the fishermen. As it increases in size great shoals are formed in the offing, though a few small are almost always found in inshore waters.

The young round fishes, such as cod, haddock, and whiting, of similar or nearly similar size, seem respectively to herd together. Thus it happens that in certain hauls of both liners and trawlers the majority agree in size. This is well known to the liners, who in former days specially sought out the young cod as already indicated. The same feature is observed in many other fishes, and probably conduces to their safety.

So far as known, the adult fishes of the three kinds specially alluded to in the preceding paragraph (viz., cod, haddock, and whiting) follow no very definite law in regard to migrations, if we except the apparent congregation in certain regions during the spawning season, as pointed out, for instance by Sars, off Lofoten, where they occur in vast numbers from January to March. In our own country, again, the appearance of shoals of haddocks and whiting in certain localities is another example. How far such multitudes, however, are influenced by the abundance of food is still an open question. In British seas the herring is the main cause of these congregations in the cod and haddock; the former chiefly pursuing the fishes, the latter their eggs. In the same way, the abundance of Norway lobsters and similar food on the grounds called banks exercise considerable influence on the presence of cod.

It has already been pointed out, however, that in their young stages certain migrations do occur. Thus the post-larval cod by-and-by seeks the laminarian region, while the older forms for the most part tend to go seaward. The same occurs even in a more pronounced manner with the ling, the adults of which as a rule are found in deep water. The pelagic post-larval ling seeks downwards as it grows, and is seldom found near the shore till it attains the length of six or seven inches, in short, until it is barred with pigment. As it increases in size it migrates seaward. Similar features are noticed in the plaice. As observed in the trawling expeditions of 1884, only large plaice as a rule are procured in deep water off the east coast, while the sandy bays abound with those ranging from 11 inches downward, and none of the females of which appear to be mature. Multitudes of little plaice haunt the margins of these sandy beaches, but it cannot be said that forms which have the length, for example, of 3 inches, are confined to any particular line drawn across a bay, for small forms (2–4 in.) occur in hauls all over such a bay as that of St. Andrews. Small turbot and halibut in the same way are often found in the shallow bays, while the large adults are inhabitants of the

deeper water. Such would not, however, seem to be the case with certain skate, very large adults of which occur in the shallow water of the sandy voes in Shetland.

On the other hand the witch (*Pleuronectes cynoglossus*) keeps to its special areas, both as regards the young and the adult condition, so that the movements of eggs, larval and post-larval forms, are circumscribed; and the same would seem to be the case with the topknot (*Zeugopterus*) and sail-fluke (*Arnoglossus*). The dab (*Pleuronectes limanda*), again, is found in all stages both in comparatively deep and in comparatively shallow water.

Almost all our valuable food-fishes, therefore, are produced from minute pelagic eggs, the enormous numbers of which provide for a vast increase and wide distribution of the species; yet it cannot be said that this habit alone provides for their multiplication when the case of the herring with its demersal eggs, fixed firmly to the bottom, is considered. It has to be borne in mind, however, that the larval herring immediately mounts upward toward the surface as soon as its strength suffices.

Many striking changes occur during growth, both in external form and coloration, but it is difficult at present to lay down any general law that would apply to all cases, though those in which certain migrations take place during growth show such changes very prominently. The young round fishes by-and-by roam about the sea in shoals, led hither and thither mainly by the presence of food; though in the case of the larger and adult forms, safety or freedom from molestation may have some influence. Though so minute on escaping from the egg, their growth is, by-and-by, rapid, and the duration of life in such as the cod is considerable. Abundance of food, more than any special instinct, would appear to be the main cause of their migrations in the adult or semi-adult state, and that food is as varied as their haunts; in short it embraces every subkingdom up to their own, for fishes and their eggs form a large share of their diet.

There would be little difficulty in adding to the sea great numbers of larval forms of any species of which eggs can be procured: yet if a few adults can be obtained in such waters at the proper season it is still an open question whether the natural process with its surroundings would not be more successful.

In the foregoing remarks I have but touched on a few of the leading features of the life-history of a food-fish; for the subject is one of vast extent, and some of the points embraced in it are by no means easily solved. We have only earnestly entered on the study of the subject in this country within the last few years, and much yet remains to be done, even in some of the most common marine fishes. However, the zoological investigator is here stimulated by the fact that all his labours directly bear on the public welfare, for it need hardly be pointed out that a thorough knowledge of the development and life-histories of our food-fishes is the first step to sound legisla-

tion and effective administration. The State has in past years spent princely sums on more or less pure science, as in the memorable voyage of the *Challenger*. There can be no doubt that at the present moment the public interests demand a searching and long-continued inquiry nearer home, viz. the exhaustive investigation of all that pertains to the food-fishes of our shores, since the problems connected therewith affect the prosperity of so large a portion of the population.

[W. C. M.]

GENERAL MONTHLY MEETING,

Monday, February 4, 1889.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Vice-President, in the Chair.

John Tomlinson Brunner, Esq. M.P.
Augustus Stroh, Esq.
Sir Charles Tennant, Bart.
John Tennant, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mr. William Anderson for his present of a portrait of Professor Mendeleef.

The Special Thanks of the Members were returned for the following Donation to the Fund for the promotion of Experimental Research:

Professor Dewar £50

The decease of Sir Frederick Pollock, Bart. Manager and Vice-President, on the 24th December last, was announced.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Bibliography of Indian Geology. By R. D. Oldham. 8vo. 1888.
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Engineer for Dec. 1888 and Jan. 1889. fol.
Engineering for Dec. 1888 and Jan. 1889. fol.
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Iron for Dec. 1888 and Jan. 1889. 4to.
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Nature for Dec. 1888 and Jan. 1889. 4to.
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Revue Scientifique for Dec. 1888 and Jan. 1889. 4to.
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Studies in Historical and Political Science, Seventh Series, No. 1. 8vo. 1889.
Kew Observatory—Report, 1888. 8vo.
Linnean Society—Journal, Nos. 156, 157, 164. 8vo. 1888.
Manchester Geological Society—Transactions, Vol. XX. Part 1. 8vo. 1888.
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Society of Architects—Proceedings, Vol. I. Nos. 3, 4, 5. 8vo. 1889.
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Telegraph Engineers, Society of—Journal, Nos. 75, 76. 8vo. 1888.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1888: Heft 9, 10. 4to.
Wells, Sir Spencer, Bart. F.R.C.S. M.R.I. (the Author)—Cancer and Cancerous Diseases. (Morton Lecture, 1888.) 8vo. 1889.
Wild, Dr. H. (the Director)—Annalen der Physikalischen Central-Observatoriums. 1887, Theil II. 4to. 1888.

WEEKLY EVENING MEETING,

Friday, February 8, 1889.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Sir William Thomson, D.C.L. LL.D. F.R.S. *M.R.I.*

Electrostatic Measurement.

(Abstract deferred).

WEEKLY EVENING MEETING,

Friday, February 15, 1889.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

Professor A. W. Rücker, M.A. F.R.S. *M.R.I.*

Electrical Stress.

The subject of the discourse was brought before the members of the Royal Institution some years ago by Mr. Gordon. In the interval a considerable amount of work has been done upon it, both in England and Germany, and many experiments have been devised to illustrate it. Some of the more striking of these, though of great interest to the student, are rarely or never shown in courses of experimental lectures. The lecturer and Mr. C. V. Boys, F.R.S., last year devised a set of apparatus which has made the optical demonstration of electrical stress comparatively easy, and most of the results obtained by Kerr and Quincke can now be demonstrated to audiences of a considerable size. Before discussing this portion of his subject the lecturer introduced it by an explanation of principles on which the experiments are founded.

Magnetic lines of force can easily be mapped out by iron filings, but the exhibition of electrical lines of force in a liquid is a more complex matter. In the first place, if two oppositely electrified bodies are introduced into a liquid which is a fairly good non-conductor, convective conduction is set up. Streams of electrified liquid pass from the one to the other. The highly refracting liquid phenyl thiocarbamide appears to be specially suitable for experiments on this subject. If an electrified point is brought over the surface a dimple is formed which becomes deeper as the point approaches it. At the instant at which the needle touches the liquid the dimple disappears, but a bubble of air from the lower end frequently remains imprisoned in the vortex caused by the downward rush of the electrified liquid from the point. It oscillates a short distance below the point, and indicates clearly the rapid motions which are produced in the fluid in its neighbourhood. When the needle is withdrawn a small column of liquid adheres to it. This effect is, however, seen to greater advantage if a sphere about 5 mm. in diameter is used instead of the needle-point. When this is withdrawn a column of liquid about 5 mm. high and 2 mm. in diameter is formed between the sphere and the surface. A similar experiment was made by Faraday on a much larger scale with oil of turpentine, and he detected the existence of currents which are in accord with the view that the unelectrified liquid flows up the exterior

of the cylinder, becomes electrified by contact, and is repelled down its axis. In view of this explanation, and the movements assumed can be clearly seen in the phenyl thiocarbamide, the performance of the experiment on a small scale is not without interest. The possibility of the formation of such violent up-and-down currents in so small a space must depend upon a very nice adjustment between the properties of the liquid and the forces in play. It is also obvious that such movements of the liquid must be a disturbing element in any attempt to make the lines of electric force visible.

Again, if a solid powder be suspended in a liquid into which electrified solids are introduced, it tends to accumulate round one of the poles. This subject has been investigated by W. Holtz. Sometimes the powder appears to move in a direction opposed to that in which the liquid is streaming. Sometimes two powders will travel towards different poles.

If powdered antimony sulphide be placed in ether, it settles at the bottom of the liquid, and if either two wires insulated with glass up to their points, or two vertical plates be used as electrodes, and slightly electrified, the solid particles arrange themselves along the lines of force. If the electrification be increased, they cluster round the positive pole. On suddenly reversing the electrification by means of a commutator, they stream along lines of force to the pole from which they were previously repelled. Other methods of obtaining the lines of force have been devised. They can, for instance, be shown by crystals of sulphate of quinine immersed in turpentine.

The tendency of the lines of force to separate one from the other was illustrated by Quincke's experiment. A bubble of air is formed in bisulphide of carbon between two horizontal plates. It is in connection with a small manometer, and when the plates are oppositely excited, the electrical pressure acting at right angles to the lines of force, being greater in the liquid than in air, compels the bubble to contract and depresses the manometer.

Kerr's experiments depend upon the fact that, since the electrical stress is a tension along the lines of force, and a pressure at right angles to them, a substance in which such a stress is produced assumes a semicrystalline condition in the sense that its properties along, and perpendicular to, the lines of force are different. Light is therefore transmitted with different velocities according as the direction of vibration coincides with, or is perpendicular to, these lines; and the familiar phenomena of the passage of polarised light through crystals may be imitated by an electrically stressed liquid.

The bisulphide of carbon used must be dry, and, to make the phenomena clearly visible, it is necessary that the light should travel through a considerable thickness. Thus, to represent the stress between two spheres, elongated parallel cylinders should be used, the axes of which are parallel to the course of the rays of light. These appear on the screen as two dark circles. Between crossed Nicols, the planes of polarisation of which are inclined at 45° to the hori-

zontal, the field is dark until the cylinders are electrified, when light is restored in the space between them.

If parallel plates with carefully rounded edges, and about 2 millimetres apart, are used, the colours of Newton's rings appear in turn, the red of the third order being sometimes reached. If one plate is convex towards the other, the colours of the higher orders appear in the middle, and travel outwards as the stress is increased. The experiments may be varied by using two concentric cylinders, or two sheets of metal bent twice at right angles to represent a section through a Leyden jar. In the first case a black cross is formed; and in the second, black brushes unite the lower angles of the images of the edges of the plates. By the interposition of a piece of selenite, which shows the blue of the second order, two of the quadrants contained between the arms of the cross become green, and the others red. In like manner the horizontal and vertical spaces between the inner and outer coatings of the "jar" become differently coloured.

There are several phenomena connected with the stress in insulators which present considerable difficulties. Thus it is found impossible to restore the light between crossed Nicols by subjecting a solid placed between them to electrical stress in a uniform field. That the non-uniformity of the field has nothing to do with the phenomenon in liquids, though at first disputed, is now generally admitted. It may be readily proved by means of a Franklin's pane, of which half is pierced with windows. The glow is much weakened by thus replacing a uniform by a non-uniform field.

Again, though most dielectrics when placed in an electric field expand, the fatty oils contract. Prof. J. J. Thomson has recently pointed out that this indicates that another set of strains are superposed upon those assumed in the ordinary explanations of these phenomena, and by which they may be neutralised or overcome.

In experiments with carbon bisulphide it is necessary to take every precaution against fire. For this purpose the cell which contains the liquid should be immersed in a larger cell, so that if—as sometimes happens—the passage of a spark cracks the glass the liquid may flow into a confined space. This should stand in a tray with turned-up edges, and an extinguisher of tin plate should be at hand to place over the whole apparatus. No Leyden jars should be included in the electrical circuit. The difficulties which formerly arose in the exhibition of experiments in statical electricity owing to the presence of moisture in the air of a lecture-room are now immensely reduced by the Wimshurst machine, which works with unfailing certainty under adverse conditions. A new and very beautiful machine was kindly lent by Mr. Wimshurst for the purposes of the lecture.

[A. W. R.]

WEEKLY EVENING MEETING,

Friday, February 22, 1889.

COLONEL J. A. GRANT, C.B. C.S.I. F.R.S. Vice-President, in the Chair.

HAROLD CRICHTON BROWNE, Esq. F.R.G.S.

In the Heart of the Atlas.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, March 1, 1889.

JOHN RAE, M.D. LL.D. F.R.S. Vice-President, in the Chair.

EDMUND GOSSE, M.A. Clark Lecturer in English Literature, Trinity College, Cambridge.

Leigh Hunt.

THE lecturer began by a consideration of Leigh Hunt's present position in the history of literature. He then analysed his personal character, illustrating it with several unpublished anecdotes. He proceeded to review Leigh Hunt's life, mainly from the point of view of his literary activity, and briefly described in detail his leading works in prose and verse. In conclusion, he made a critical examination of Hunt's style at various periods of his career, and summed up with a *résumé* of his principal merits as a prose-writer and as a poet.

GENERAL MONTHLY MEETING,

Monday, March 4, 1889.

SIR JAMES CRICHTON BROWNE, M.D. LL.D. F.R.S. Vice-President, in the Chair.

John Cope Butterfield, Esq. F.C.S.
Charles Cave, Esq.
Mrs. E. S. Dewick,
Miss K. D. Doulton,
Joseph Gordon Gordon, Esq.

Mrs. Sibble Heap,
Miss Mona Kemp,
Lieutenant Nathaniel J. Lyon,
Alfred Nobel, Esq.
Richard Douglas Powell, M.D. F.R.C.P.
Thomas James Reeves, Esq. B.A. F.Z.S.
Mrs. W. Chandler Roberts-Austen,
Mrs. H. Sutton,
George Edward Whitton, Esq. M.B.
Ernest H. Winstone, Esq. M.A.
Mrs. M. A. E. Wright,

were elected Members of the Royal Institution.

The Honorary Secretary reported, That the following Resolution of the Managers respecting Sir Frederick Pollock, Bart. had been communicated to Lady Pollock, and then read her Ladyship's acknowledgment:—

Resolved, "That the Managers of the Royal Institution of Great Britain desire at this their first Meeting after the death of their lamented friend and colleague, Sir Frederick Pollock, to record their sense of the great loss sustained by the Institution and by themselves, in that they are for ever deprived of the advice and assistance of a colleague who had been for over forty years a Member of the Institution, and who, during that time, had devoted himself assiduously to its interests, not only in his capacity as a Member, but as a Manager and Vice-President.

"Sir Frederick Pollock's varied scholarly attainments, combined with his intimate acquaintance with men distinguished in literature and art, enabled him to render special services most valuable to the Institution; whilst his kindly and courteous demeanour endeared him to all those with whom he was for so many years associated.

"The Managers further desire to be permitted to offer to Lady Pollock the expression of their most sincere sympathy and condolence with her in her bereavement."

Resolved, "That the Honorary Secretary do send to Lady Pollock a copy of this Resolution."

"59, Montagu Square, W. *February 8th*, 1889.

"Juliet Lady Pollock presents her compliments to Sir Frederick Bramwell and his colleagues, and begs to thank them very warmly and sincerely for their appreciation so cordially expressed of her dear husband's distinguished qualities, and for their sympathy with her sorrow."

The Honorary Secretary further reported, That the following letter had been read to the Meeting of Managers at their Meeting this day:—

"Albemarle Street, W. 26*th February*, 1889.

"Dear Sir Frederick,

"I write now with much regret to request you to be so good as to inform the Managers at their next Meeting, on Monday, the 4th of March, that I feel it my duty, in consequence of failing sight, to resign the offices which I now hold in the Royal Institution as Assistant Secretary and Librarian, and that I await their decision as to the most convenient time for retiring.

"I trust you will excuse my mentioning the following facts. In consequence of the recommendation of Professor Faraday, who had known me from my youth,

I was engaged by the Managers as Assistant Secretary in May 1848, and appointed Librarian in the following December. As lasting memorials of my long connection with the Institution, I would refer to the Index of the Meetings of the Managers and Members, beginning in 1799; to the two volumes of the Classified Catalogue of the Library, published in 1857 and 1882; to the eleven volumes of the 'Proceedings of the Royal Institution,' the printing of which was authorised by the Managers at my suggestion in 1851, and which were long edited by me, and more recently by Mr. Henry Young, under my direction; and to the nine Stands of Books of Reference, collected and placed in the Upper Library.

"In conclusion I desire to express my grateful sense of the uniform kindness which I have received from the Members, the Committees, and all the Honorary Secretaries whom I have served. My hearty desire during the greater part of my life has been to promote the objects of the Institution and the comfort of its Members.

"I remain, Sir Frederick,
"With many thanks for your own personal kindness,
"Yours faithfully,
"BENJAMIN VINCENT."

The Honorary Secretary further reported, That the following Resolution had been passed by the Managers:—

Resolved, "That the Managers accept Mr. Vincent's resignation of the offices of Assistant Secretary and Keeper of the Library, and, as a mark of their high appreciation of the valuable services he has rendered the Institution, elect him to the office of Honorary Librarian, at a salary of 150*l*. a year, with all the privileges of the Institution."

Mr. Henry Young was appointed by the Managers Assistant Secretary and Keeper of the Library at their Meeting this day.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz:—

FROM

The New Zealand Government—Statistics of the Colony of New Zealand, 1887. fol. 1888.
The French Government—Documents Inedits sur l'Histoire de France: Recueil des Chartes de l'Abbaye de Cluny, Tome IV. 4to. 1888.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 2° Semestre, Vol. IV. Fasc. 6, 7, 8, 9, 10. 8vo. 1888.
Astronomical Society, Royal—Monthly Notices, Vol. XLIX. No. 3. 8vo. 1889.
Bankers, Institute of—Journal, Vol. X. Part 2. 8vo. 1889.
British Architects, Royal Institute of—Proceedings, 1888-9, Nos. 8, 9. 4to.
Brymner, Douglas, Esq. (the Archivist)—Report on Canadian Archives, 1888. 8vo. 1889.
Chadwick, Edwin, Esq. C.B.—The Health of Nations: a Review of the Works of Edwin Chadwick, with a Biography. By B. W. Richardson. 2 vol. 8vo. 1887.
Chemical Industry, Society of—Journal, Vol. VIII. No. 1. 8vo. 1889.
Chemical Society—Journal for February, 1889. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, 1888, Part 6a; 1889, Part 1. 8vo.
East India Association—Journal, Vol. XXI. No. 1. 8vo. 1889.
Editors—American Journal of Science for March, 1889. 8vo.
Analyst for March, 1889. 8vo.
Athenæum for March, 1889. 4to.
Chemical News for March, 1889. 4to.

Editors—Chemist and Druggist for March, 1889. 8vo.
Electrical Engineer for March, 1889.
Engineer for March, 1889. fol.
Engineering for March, 1889. fol.
Horological Journal for March, 1889. 8vo.
Industries for March, 1889. fol.
Iron for March, 1889. 4to.
Murray's Magazine for March, 1889. 8vo.
Nature for March, 1889. 4to.
Photographic News for March, 1889. 8vo.
Revue Scientifique for March, 1889. 4to.
Telegraphic Journal for March, 1889. 8vo.
Zoophilist for March, 1889. 4to.
Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 74. 8vo. 1888.
Franklin Institute—Journal, No. 758. 8vo. 1889.
Geneva, Société de Physique et d'Histoire Naturelle—Memoires, Tome XXX. Partie 1. 4to. 1888.
Geological Institute, Imperial, Vienna—Verhandlungen, 1888, Nos. 15–18; 1889, No. 1. 8vo.
Geological Society—Quarterly Journal, No. 177. 8vo. 1889.
Geographical Society, Royal—Proceedings, New Series, Vol. XI. No. 2. 8vo. 1889.
Georgofili, Reale Accademia—Atti, Vol. X. Disp. 4 and Sup. 8vo. 1888–9.
Johns Hopkins University—American Chemical Journal, Vol. X. Nos. 4, 5, 6. 8vo. 1888.
American Journal of Philology, Nos. 34, 35. 8vo. 1888.
Studies in Historical and Political Science. Sixth Series, and Seventh Series, No. 1. 8vo. 1888–9.
Linnean Society—Journal, Nos. 121, 165-169. 8vo. 1889.
Manchester Geological Society—Transactions, Vol. XX. Parts 2, 3. 8vo. 1889.
Mechanical Engineers' Institution—Proceedings, 1888, No. 4. 8vo.
Meteorological Office—Hourly Readings, 1886, Part 1. 4to. 1889.
Report of the Meteorological Council, RS. 31 March, 1888. 8vo.
Weekly Weather Reports, October-December, 1888. 4to.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quinta, Vol. II. No. 12. 8vo. And Disegni. fol. 1888.
Numismatic Society—Chronicle and Journal, 1888, Part 4. 8vo. 1888.
Odontological Society of Great Britain—Transactions, Vol. XXI. Nos. 3, 4. New Series. 8vo. 1888.
Pharmaceutical Society of Great Britain—Journal, February, 1889. 8vo.
Photographic Society—Journal, Vol. XIII. No. 4. 8vo. 1889.
Richardson, B. W. M.D. F.R.S. M.R.I. (the Author)—The Asclepiad, Vol. V. No. 21. 8vo. 1889.
Rio de Janeiro Observatory—Revista, No, 1. 8vo. 1889.
Royal Irish Academy—Transactions, Vol. XXIX. Part 5. 4to. 1888.
Royal Society of London—Proceedings, Nos. 274, 275. 8vo. 1889.
Philosophical Transactions, Vol. CLXXIX. 4to. 1889.
Scottish Society of Arts, Royal—Transactions, Vol. XII. Part 2. 8vo. 1889.
Society of Architects—Proceedings, Vol. I. No. 7. 8vo. 1889.
Society of Arts—Journal, February, 1889. 8vo.
St. Bartholomew's Hospital—Reports, Vol. XXIV. 8vo. 1888.
Telegraph Engineers, Society of—Journal, No. 77. 8vo. 1889.
United Service Institution, Royal—Journal, No. 146. 8vo. 1889.
Vereins zur Beförderung des Gewerb eisses in Preussen—Verhandlungen, 1889: Heft 1. 4to.
Victoria Institute—Transactions, No. 87. 8vo. 1889.
Yorkshire Archæological and Topographical Association—Journal, Part XL. 8vo. 1889.

WEEKLY EVENING MEETING,

Friday, March 8, 1889,

Sir James Crichton Browne, M.D. LL.D. F.R.S.
Vice-President, in the Chair.

Professor Oliver Lodge, LL.D. F.R.S.

The Discharge of a Leyden Jar.

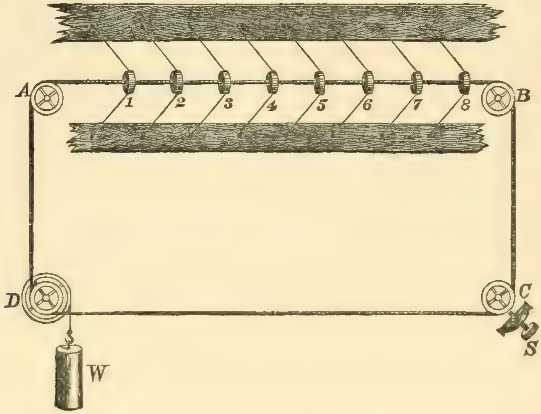
It is one of the great generalisations established by Faraday, that all electrical charge and discharge is essentially the charge and discharge of a Leyden jar. It is impossible to charge one body alone. Whenever a body is charged positively, some other body is *ipso facto* charged negatively, and the two equal opposite charges are connected by lines of induction. The charges are, in fact, simply the ends of these lines, and it is as impossible to have one charge without its correlative as it is to have one end of a piece of string without there being somewhere, hidden it may be, split up into strands it may be, but somewhere existent, the other end of that string.

This I suppose familiar fact that all charge is virtually that of a Leyden jar being premised, our subject for this evening is at once seen to be a very wide one, ranging in fact over the whole domain of electricity. For the charge of a Leyden jar includes virtually the domain of electrostatics; while the discharge of a jar, since it constitutes a current, covers the ground of current electricity all except that portion which deals with phenomena peculiar to steady currents. And since a current of electricity necessarily magnetises the space around it, whether it flow in a straight or in a curved path, whether it flow through wire or burst through air, the territory of magnetism is likewise invaded; and inasmuch as a Leyden jar discharge is oscillatory, and we now know the vibratory motion called light to be really an oscillating electric current, the domain of optics is seriously encroached upon.

But though the subject I have chosen would permit this wide range, and though it is highly desirable to keep before our minds the wide-reaching import of the most simple-seeming fact in connection with such a subject, yet to-night I do not intend to avail myself of any such latitude, but to keep as closely and distinctly as possible to the Leyden jar in its homely and well-known form, as constructed out of a glass bottle, two sheets of tinfoil, and some stickphast.

The act of charging such a jar I have permitted myself now for some time to illustrate by the mechanical analogy of an inextensible endless cord able to circulate over pulleys, and threading in some portion of its length a row of tightly-gripping beads which are connected to fixed beams by elastic threads.

The cord is to represent electricity; the beads represent successive strata in the thickness of the glass of the jar, or, if you like, atoms of dielectric or insulating matter. Extra tension in the cord represents negative potential, while a less tension (the nearest analogue to pressure adapted to the circumstances) represents positive



Mechanical analogy of a circuit partly *dielectric*; for instance, of a charged condenser. A is its positive coat, B its negative.

potential. Forces applied to move the cord, such as winches or weights, are electromotive forces; a clamp or fixed obstruction represents a rheostat or contact-breaker; and an excess or defect of cord between two strata of matter represents a positive or a negative charge.

The act of charging a jar is now quite easily depicted as shown in the diagram.

To discharge the jar one must remove the charging E.M.F. and unclamp the screw, i.e. close the circuit. The stress in the elastic threads will then rapidly drive the cord back, the inertia of the beads will cause it to overshoot the mark, and for an instant the jar will possess an inverse charge. Back again the cord swings, however, and a charge of same sign as at first, but of rather less magnitude, would be found in the jar, if the operation were now suspended. If it be allowed to go on, the oscillations gradually subside, and in a short time everything is quiescent, and the jar is completely discharged.

All this occurs in the Leyden jar, and the whole series of oscillations, accompanied by periodic reversal and re-reversal of the charges of the jar, is all accomplished in the incredibly short space of time occupied by a spark.

Consider now what the rate of oscillation depends on. Manifestly on the elasticity of the threads and on the inertia of the matter which

is moved. Take the simplest mechanical analogy, that of the vibration of a loaded spring, like the reeds in a musical box. The stiffer the spring, and the less the load, the faster it vibrates. Give a mathematician these data, and he will calculate for you the time the spring takes to execute one complete vibration, the "period" of its swing. [Loaded lath in vice.]

The electrical problem and the electrical solution are precisely the same. That which corresponds to the flexibility of the spring, is in electrical language called static capacity, or, by Mr. Heaviside, permittance. That which corresponds to the inertia of ordinary matter is called electro-magnetic inertia, or self-induction, or, by Mr. Heaviside, inductance.

Increase either of these, and the rate of oscillation is diminished. Increasing the static capacity corresponds to lengthening the spring; increasing the self-induction corresponds to loading it.

Now the static capacity is increased simply by using a larger jar, or by combining a number of jars into a battery in the very old established way. Increase in the self-induction is attained by giving the discharge more space to magnetise, or by making it magnetise a given space more strongly. For electro-magnetic inertia is wholly due to the magnetisation of the space surrounding a current, and this space may be increased or its magnetisation intensified as much as we please.

To increase the space we have only to make the discharge take a long circuit instead of a short one. Thus we may send it by a wire all round the room, or by a telegraph wire all round a town, and all the space inside it and some of that outside will be more or less magnetised. More or less, I say, as it is a case of less rather than more. Practically very little effect is felt except close to the conductor, and accordingly the self-induction increases very nearly proportionally to the length of the wire, and not in proportion to the area inclosed: provided also the going and return wires are kept a reasonable distance apart, so as not to encroach upon each other's appreciably magnetised regions.

But it is just as effective, and more compact, to intensify the magnetisation of a given space by sending the current hundreds of times round it instead of only once; and this is done by inserting a coil of wire into the discharge circuit.

Yet a third way there is of increasing the magnetisation of a given space, and that is to fill it with some very magnetisable substance such as iron. This, indeed, is a most powerful method under many circumstances, it being possible to increase the magnetisation and therefore the self-induction or inertia of the current some 5000 times by the use of iron.

But in the case of the discharge of a Leyden jar, iron is of no advantage. The current oscillates so quickly that any iron introduced into its circuit, however subdivided into thin wires it may be, is protected from magnetism by inverse currents induced in its outer

skin, as your Professor of Natural Philosophy* has shown, and accordingly it does not get magnetised ; and so far from increasing the inductance of the discharge circuit it positively diminishes it by the reaction effect of these induced currents: it acts, in fact, much as a mass of copper might be expected to do.

The conditions determining rate of oscillation being understood we have next to consider what regulates the damping out of the vibrations, i. e. the total duration of the discharge.

Resistance is one thing. To check the oscillations of a vibrating spring you apply to it friction, or make it move in a viscous medium, and its vibrations are speedily damped out. The friction may be made so great that oscillations are entirely prevented, the motion being a mere dead-beat return to the position of equilibrium; or, again, it may be greater still, and the motion may correspond to a mere leak or slow sliding back, taking hours or days for its accomplishment, With very large condensers, such as are used in telegraphy, this kind of discharge is frequent, but in the case of a Leyden jar discharge it is entirely exceptional. It can be caused by including in the circuit a wet string, or a capillary tube full of distilled water, or a slab of wood, or other atrociously bad conductor of that sort; but the conditions ordinarily associated with the discharge of a Leyden jar, whether it discharge through a long or a short wire, or simply through its tongs, or whether it overflow its edge or puncture its glass, are such as correspond to oscillations, and not to leak. [Discharge jar first through wire and next through wood.]

When the jar is made to leak through wood or water the discharge is found to be still not steady : it is not oscillatory indeed, but it is intermittent. It occurs in a series of little jerks, as when a thing is made to slide over a resined surface. The reason of this is that the terminals discharge faster than the circuit can supply the electricity, and so the flow is continually stopped and begun again.

Such a discharge as this, consisting really of a succession of small sparks, may readily appeal to the eye as a single flash, but it lacks the noise and violence of the ordinary discharge; and any kind of moving mirror will easily analyse it into its constituents and show it to be intermittent. [Shake a mirror, or waggle head, or opera-glass.]

It is pretty safe to say, then, that whenever a jar discharge is not oscillatory it is intermittent, and when not intermittent is oscillatory. There is an intermediate case when it is really dead-beat, but it could only be hit upon with special care, while its occurrence by accident must be rare.

So far I have only mentioned resistance or friction as the cause of the dying out of the vibrations; but there is another cause, and that a most exciting one.

The vibrations of a reed are damped partly indeed by friction and

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* Lord Rayleigh.

imperfect elasticity, but partly also by the energy transferred to the surrounding medium and consumed in the production of sound. It is the formation and propagation of sound waves which largely damp out the vibrations of any musical instrument. So it is also in electricity. The oscillatory discharge of a Leyden jar disturbs the medium surrounding it, carves it into waves which travel away from it into space: travel with a velocity of 185,000 miles a second: travel precisely with the velocity of light. [Tuning-fork.]

The second cause, then, which damps out the oscillations in a discharge circuit is *radiation: electrical* radiation if you like so to distinguish it, but it differs in no respect from ordinary radiation (or "radiant heat" as it has so often been called in this place); it differs in no respect from Light except in the physiological fact that the retinal mechanism, whatever it may be, responds only to waves of a particular, and that a very small, size, while radiation in general may have waves which range from 10,000 miles to a millionth of an inch in length.

The seeds of this great discovery of the nature of light were sown in this place: it is all the outcome of Faraday's magneto-electric and electrostatic induction: the development of them into a rich and full-blown theory was the greatest part of the life-work of Clerk-Maxwell: the harvest of experimental verification is now being reaped by a German. But by no ordinary German. Dr. Hertz, now Professor in the Polytechnicum of Karlsrühe, is a young investigator of the highest type. Trained in the school of Helmholtz, and endowed with both mathematical knowledge and great experimental skill, he has immortalised himself by a brilliant series of investigations which have cut right into the ripe corn of scientific opinion in these islands, and by the same strokes as have harvested the grain have opened up wide and many branching avenues to other investigators.

At one time I had thought of addressing you this evening on the subject of these researches of Hertz, but the experiments are not yet reproducible on a scale suited to a large audience, and I have been so closely occupied with some not wholly dissimilar, but independently conducted, researches of my own—researches led up to through the unlikely avenue of lightning-conductors—that I have had as yet no time to do more than verify some of them for my own edification.

In this work of repetition and verification Prof. Fitzgerald has, as related in a recent number of NATURE (February 21, p. 391), probably gone further; and if I may venture a suggestion to your Honorary Secretary, I feel sure that a discourse on Hertz's researches from Prof. Fitzgerald next year would be not only acceptable to you, but would be highly conducive to the progress of science.

I have wandered a little from my Leyden jar, and I must return to it and its oscillations. Let me very briefly run over the history of our knowledge of the oscillatory character of a Leyden jar discharge. It was first clearly realised and distinctly stated by that excellent

experimentalist, Joseph Henry, of Washington, a man not wholly unlike Faraday in his mode of work, though doubtless possessing to a less degree that astonishing insight into intricate and obscure phenomena; wanting also in Faraday's circumstantial advantages.

This great man arrived at a conviction that the Leyden jar discharge was oscillatory by studying the singular phenomena attending the magnetisation of steel needles by a Leyden jar discharge, first observed in 1824 by Savary. Fine needles, when taken out of the magnetising helices, were found to be not always magnetised in the right direction, and the subject is referred to in German books as anomalous magnetisation. It is not the magnetisation which is anomalous, but the currents which have no simple direction; and we find in a memoir published by Henry in 1842, the following words:—

"This anomaly, which has remained so long unexplained, and which, at first sight, appears at variance with all our theoretical ideas of the connection of electricity and magnetism, was, after considerable study, satisfactorily referred by the author to an action of the discharge of the Leyden jar, which had never before been recognised. The discharge, whatever may be its nature, is not correctly represented (employing for simplicity the theory of Franklin) by the single transfer of an imponderable fluid from one side of the jar to the other; the phenomenon requires us to admit *the existence of a principal discharge in one direction and then several reflex actions backward and forward each more feeble than the preceding, until the equilibrium is obtained.* All the facts are shown to be in accordance with this hypothesis, and a ready explanation is afforded by it of a number of phenomena, which are to be found in the older works on electricity, but which have until this time remained unexplained."*

The italics are Henry's. Now if this were an isolated passage it might be nothing more than a lucky guess. But it is not. The conclusion is one at which he arrives after a laborious repetition and serious study of the facts, and he keeps the idea constantly before him when once grasped, and uses it in all the rest of his researches on the subject. The facts studied by Henry do in my opinion support his conclusion, and if I am right in this it follows that he is the original discoverer of the oscillatory character of a spark, although he does not attempt to state his theory. That was first done, and completely done, in 1853, by Sir William Thomson; and the progress of experiment by Feddersen, Helmholtz, Schiller, and others has done nothing but substantiate it.

The writings of Henry have been only quite recently collected and published by the Smithsonian Institution of Washington in accessible form, and accordingly they have been far too much ignored.

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* 'Scientific Writings of Joseph Henry,' vol. i. p. 201. Published by the Smithsonian Institution, Washington, 1886.

The two volumes contain a wealth of beautiful experiments clearly recorded, and well repay perusal.

The discovery of the oscillatory character of a Leyden jar discharge may seem a small matter but it is not. One has only to recall the fact that the oscillators of Hertz are essentially Leyden jars—one has only to use the phrase "electro-magnetic theory of light"—to have some of the momentous issues of this discovery flash before one.

One more extract I must make from that same memoir by Henry,* and it is a most interesting one; it shows how near he was, or might have been, to obtaining some of the results of Hertz; though if he had obtained them, neither he nor any other experimentalist could possibly have divined their real significance.

It is, after all, the genius of Maxwell and of a few other great theoretical physicists whose names are on everyone's lips† which endows the simple induction experiments of Hertz and others with such stupendous importance.

Here is the quotation :—

"In extending the researches relative to this part of the investigations, a remarkable result was obtained in regard to the distance at which induction effects are produced by a very small quantity of electricity; a single spark from the prime conductor of a machine, of about an inch long, thrown on to the end of a circuit of wire in an upper room, produced an induction sufficiently powerful to magnetise needles in a parallel circuit of iron placed in the cellar beneath, at a perpendicular distance of 30 feet, with two floors and ceilings, each 14 inches thick, intervening. The author is disposed to adopt the hypothesis of an electrical *plenum* [in other words, of an ether], and from the foregoing experiment it would appear that a single spark is sufficient to disturb perceptibly the electricity of space throughout at least a cube of 400,000 feet of capacity; and when it is considered that the magnetism of the needle is the result of the difference of two actions, it may be further inferred that the diffusion of motion in this case is almost comparable with that of a spark from a flint and steel in the case of light."

Comparable it is, indeed, for we now know it to be the self-same process.

One immediate consequence and easy proof of the oscillatory character of a Leyden jar discharge is the occurrence of phenomena of sympathetic resonance.

Everyone knows that one tuning-fork can excite another at a

* Loc. cit., p. 204.

† And of one whose name is not yet on everybody's lips, but whose profound researches into electro-magnetic waves have penetrated further than anybody yet understands into the depths of the subject, and whose papers have very likely contributed largely to the theoretical inspiration of Hertz—I mean that powerful mathematical physicist, Mr. Oliver Heaviside.

reasonable distance if both are tuned to the same note. Everyone knows, also, that a fork can throw a stretched string attached to it into sympathetic vibration if the two are tuned to unison or to some simple harmonic. Both these facts have their electrical analogue. I have not time to go fully into the matter to-night, but I may just mention the two cases which I have myself specially noticed.

A Leyden jar discharge can so excite a similarly-timed neighbouring Leyden jar circuit as to cause the latter to burst its dielectric if thin and weak enough. The well-timed impulses accumulate in the neighbouring circuit till they break through a quite perceptible thickness of air.

Put the circuits out of unison by varying the capacity or by including a longer wire in one of them; then, although the added wire be a coil of several turns, well adapted to assist mutual induction as ordinarily understood, the effect will no longer occur, until the capacity is suitably diminished and the synchronism thus restored.

That is one case, and it is the electrical analogue of one tuning-fork exciting another. It is too small at present to show here satisfactorily, for I only recently observed it, but it is exhibited in the library at the back.

The other case, analogous to the excitation of a stretched string of proper length by a tuning-fork, I published last year under the name of the experiment of the recoil kick, where a Leyden jar circuit sends waves along a wire connected by one end with it, which waves splash off at the far end with an electric brush or long spark.

I will show merely one phase of it to-night, and that is the reaction of the impulse accumulated in the wire upon the jar itself, causing it to either overflow or burst. (Sparks of gallon or pint jar made to overflow by wire round room.*)

The early observations by Franklin on the bursting of Leyden jars, and the extraordinary complexity or multiplicity of the fracture that often results, are most interesting. His electric experiments as

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* During the course of this experiment, the gilt paper on the wall was observed by the audience to be sparkling, every gilt patch over a certain area discharging into the next, after the manner of a spangled jar. It was probably due to some kind of sympathetic resonance. Electricity splashes about in conductors in a surprising way everywhere in the neighbourhood of a discharge. For instance, a telescope in the hand of one of the audience was reported afterwards to be giving off little sparks at every discharge of the jar. Everything which happens to have a period of electric oscillation corresponding to some harmonic of the main oscillation of a discharge is liable to behave in this way. When light falls on an opaque surface it turns into some other form of energy. What the audience saw was probably the result of waves of electrical radiation being quenched or reflected by the walls of the room, and generating electrical currents in the act. It is these electric surgings which render such severe caution necessary in the erection of lightning-conductors.

This explanation is merely tentative. I have had no time to investigate the matter locally.

well as Henry's well repay perusal, though, of course, they belong to the infancy of the subject.

He notes the striking fact that the bursting of a jar is an extra occurrence, it does not replace the ordinary discharge in the proper place, it accompanies it; and we now know that it is precipitated by it, that the spark occurring properly between the knobs sets up such violent surgings that the jar is far more violently strained than by the static charge or mere difference of potentials between its coatings; and if the surgings are at all even roughly properly timed, the jar is bound to either overflow or burst.

Hence a jar should always be made without a lid, and with a lip protruding a carefully considered distance above its coatings: not so far as to fail to act as a safety valve, but far enough to prevent overflow under ordinary and easy circumstances.

And now we come to what is after all the main subject of my discourse this evening, viz. the optical and audible demonstration of the oscillations occurring in the Leyden jar spark. Such a demonstration has, so far as I know, never before been attempted, but if nothing goes wrong we shall easily accomplish it.

And first I will do it audibly. To this end the oscillations must be brought down from their extraordinary frequency of a million or hundred thousand a second to a rate within the limits of human audition. One does it exactly as in the case of the spring—one first increases the flexibility and then one loads it. [Spark from battery of jars and varying sound of same.]

Using the largest battery of jars at our disposal, I take the spark between these two knobs—not a long spark, $\frac{1}{4}$ inch will be quite sufficient. Notwithstanding the great capacity, the rate of vibration is still far above the limit of audibility, and nothing but the customary crack is heard. I next add inertia to the circuit by including a great coil of wire, and at once the spark changes character, becoming very shrill but an unmistakable whistle, of a quality approximating to the cry of a bat. Add another coil, and down comes the pace once more, to something like 5000 per second, or about the highest note of a piano. Again and again I load the circuit with magnetisability, and at last the spark has only 500 vibrations a second, giving the octave, or perhaps the double octave, above the middle C.

One sees clearly why one gets a musical note: the noise of the spark is due to a sudden heating of the air; now if the heat is oscillatory, the sound will be oscillatory too, but both will be an octave above the electric oscillation, if I may so express it, because two heat-pulses will accompany every complete electric vibration, the heat production being independent of direction of current.

Having thus got the frequency of oscillation down to so manageable a value, the optical analysis of it presents no difficulty: a simple looking-glass waggled in the hand will suffice to spread out the spark

into a serrated band, just as can be done with a singing or a sensitive flame, a band too of very much the same appearance.

Using an ordinary four-square rotating mirror driven electro-magnetically at the rate of some two or three revolutions per second, the band is at the lowest pitch seen to be quite coarsely serrated ; and fine serrations can be seen with four revolutions per second in even the shrill whistling sparks.

The only difficulty in seeing these effects is to catch them at the right moment. They are only visible for a minute fraction of a revolution, though the band may appear drawn out to some length. The further away the spark is from the mirror, the more drawn out it is, but also the less chance there is of catching its image.

With a single observer it is easy to arrange a contact maker on the axle of the mirror which shall bring on the discharge at the right place in the revolution, and the observer may then conveniently watch for the image in a telescope or opera-glass, though at the lower pitches nothing of the kind is necessary.

But to show it to a large audience various plans can be adopted. One is to arrange for several sparks instead of one ; another is to multiply images of a single spark by suitably adjusted reflectors, which if they are concave will give magnified images ; another is to use several rotating mirrors ; and indeed I do use two, one adjusted so as to suit the spectators in the gallery.

But the best plan that has struck me is to combine an intermittent and an oscillatory discharge. Have the circuit in two branches, one of high resistance so as to give intermittences, the other of ordinary resistance so as to be oscillatory, and let the mirror analyse every constituent of the intermittent discharge into a serrated band. There will thus be not one spark, but several successive sparks, close enough together to sound almost like one, separate enough in the rotating mirror to be visible on all sides at once, and each one analysed into its component alternations.

But to achieve this one must have great exciting power. In spite of the power of this magnificent Wimshurst machine, it takes some time to charge up our great Leyden battery, and it is tedious waiting for each spark. A Wimshurst does admirably for a single observer, but for a multitude one wants an instrument which shall charge the battery not once only but many times over, with overflows between, and all in the twinkling of an eye.

To get this I must abandon my friend Mr. Wimshurst, and return to Michael Faraday. In front of the table is a great induction coil; its secondary has the resistance needed to give an intermittent discharge. The quantity it supplies at a single spark will fill our jars to overflowing several times over. The discharge circuit and all its circumstances shall remain unchanged. [Excite jars by coil.]

Running over the gamut with this coil now used as our exciter instead of the Wimshurst machine—everything else remaining exactly as it was—you hear the sparks give the same notes as before,

but with a slight rattle in addition, indicating intermittence as well as alternation. Rotate the mirror, and everyone should see one or other of the serrated bands of light at nearly every break of the primary current of the coil. [Rotating mirror to analyse sparks.]

The musical sparks which I have now shown you were obtained by me during a special digression * which I made while examining the effect of discharging a Leyden jar round heavy glass or bisulphide of carbon. The rotation of the plane of polarisation of light by a steady current, or by a magnetic field of any kind properly disposed with respect to the rays of light, is a very familiar one in this place. Perhaps it is known also that it can be done by a Leyden jar current. But I do not think it is; and the fact seems to me very interesting. It is not exactly new—in fact, as things go now it may be almost called old, for it was investigated six or seven years ago by two most highly skilled French experimenters, Messrs. Bichat and Blondlot.

But it is exceedingly interesting as showing how short a time, how absolutely no time, is needed by heavy glass to throw itself into the suitable rotatory condition. Some observers have thought they had proved that heavy glass requires time to develop the effect, by spinning it between the poles of a magnet and seeing the effect decrease; but their conclusions cannot be right, for the polarised light follows every oscillation in a discharge, the plane of polarisation being waved to and fro as often as 70,000 times a second in my own observation.

Very few persons in the world have seen the effect. In fact, I doubt if anyone had seen it a month ago except Messrs. Bichat and Blondlot. But I hope to make it visible to most persons here, though I hardly hope to make it visible to all.

Returning to the Wimshurst machine as exciter, I pass a discharge round the spiral of wire inclosing this long tube of CS_2, and the analysing Nicol being turned to darkness, there may be seen a faint—by those close to not so faint, but a very momentary—restoration of light on the screen at every spark. (CS_2 tube experiment on screen.)

Now I say that this light restoration is also oscillatory. One way of proving this fact is to insert a biquartz between the Nicols. With a steady current it constitutes a sensitive detector of rotation, its sensitive tint turning green on one side and red on the other. But with this oscillatory current a biquartz does absolutely nothing. (Biquartz.)

That is one proof. Another is that rotating the analyser either way weakens the extra brightening of the field, and weakens it equally either way.

But the most convincing proof is to reflect the light coming

* Most likely it was a conversation which I had with Sir Wm. Thomson, at Christmas, which caused me to see the interest of getting slow oscillations. My attention has mainly been directed to getting them quick.

through the tube upon our rotating mirror, and to look now, not at the spark, or not only at the spark, but at the faint band into which the last residue of light coming through polariser and tube and analyser is drawn out. (Analyse the light in rotating mirror.)

At every discharge this faint streak brightens in places into a beaded band: these are the oscillations of the polarised light: and when examined side by side they are as absolutely synchronous with the oscillations of the spark itself as can be perceived.

Rotating the analysing Nicol a little, one sees every alternate bead grow fainter, while the other alternate ones brighten; thus directly establishing the fact of alternations, as distinct from intermittences. A certain definite rotation will obliterate one set altogether, and make the beading appear twice as coarse, as if it belonged to the octave below. [For further details see 'Philosophical Magazine' for April, 1889.]

Out of a multitude of phenomena connected with the Leyden jar discharge I have selected a few only to present to you here this evening. Many more might have been shown, and great numbers more are not at present adapted for presentation to an audience, being only visible with difficulty and close to.

An old and trite subject is seen to have in the light of theory an unexpected charm and brilliancy. So it is with a great number of other old familiar facts at the present time.

The present is an epoch of astounding activity in physical science. Progress is a thing of months and weeks, almost of days. The long line of isolated ripples of past discovery seem blending into a mighty wave, on the crest of which one begins to discern some oncoming magnificent generalisation. The suspense is becoming feverish, at times almost painful. One feels like a boy who has been long strumming on the silent key-board of a deserted organ, into the chest of which an unseen power begins to blow a vivifying breath. Astonished, he now finds that the touch of a finger elicits a responsive note, and he hesitates, half delighted, half affrighted, least he be deafened by the chords which it would seem he can now summon forth almost at will.

[O. L.]

WEEKLY EVENING MEETING,

Friday, March 15, 1889.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Sir James N. Douglass, F.R.S. *M.R.I.*

Beacon Lights and Fog Signals.

It is stated by Samuel Smiles, in his 'Lives of Engineers,' that "with Winstanley's structure on the Eddystone in 1696, may be said to have commenced the modern engineering efforts," in directing the great sources of power in nature, for the use and convenience of man; efforts, which, followed up by Rudyerd, Smeaton, and others, have been so successful in converting hidden dangers into sources of safety, and ensuring the beneficent guidance of the mariner in his trackless path.

The famous structure of Smeaton, which had withstood the storms of more than half a century with incalculable advantage to mankind, became in course of time a matter of anxiety and watchful care to the Corporation of Trinity House, owing to the great tremor of the building with each wave stroke, during heavy westerly storms. The joints of the masonry frequently yielded to the heavy strains, and the sea-water was driven through them to the interior of the building. The upper part of the structure was strengthened with internal iron-work in 1839, and again in 1865. On the last occasion, it was found that the chief mischief was caused by the upward stroke of the heavy seas against the projecting cornice of the lantern gallery, thus lifting this portion of the masonry, together with the lantern above it. Unfortunately, the portion of the gneis rock on which the lighthouse was founded, had become seriously shaken by the heavy sea strokes on the tower, and the rock had thus been seriously undermined at its base. The waves rose during storms considerably above the summit of the lantern, thus frequently eclipsing the light, and altering its distinctive character from a fixed light to an occulting. This matter of distinctive character in a beacon light, was one of little importance at the date of the erection of Smeaton's lighthouse, when coal fires were the only illuminating agents along the coasts; but with the rapid development of our commerce, and the great increase in the number of coast lights, it has become an absolute necessity that each light maintain a clearly distinctive character. It was, therefore,

determined by the Trinity House, in 1877, to erect a new lighthouse at a distance of 120 feet from Smeaton's tower, where a safe and permanent foundation was found, but at a much lower level, which necessitated the laying of a large portion of the foundation masonry below low water. The foundation stone of this work was laid on the 19th August, 1879, by H.R.H. The Duke of Edinburgh, Master of the Trinity House; assisted by H.R.H. The Prince of Wales, an honorary Elder Brother of the Corporation.

On the 1st June, 1881, H.R.H. The Master, when passing up Channel in H.M.S. *Lively*, landed at the rock and laid the last stone of the tower; and on the 18th May of the following year H.R.H. lighted the lamps, and formally opened the lighthouse. The edifice was thus completed within four years from its commencement, at a cost of 59,255*l.* The work was executed under the immediate direction of the Trinity House and their engineer, and with a saving of 24,000*l.* on the lowest sum at which it had been found that it could be executed by contract. Every block of granite in the structure, is dovetailed together, both vertically and horizontally, on a system devised by my father, and first adopted at the Hanois rock lighthouse, off the west coast of Guernsey. The illuminating apparatus consists of two superposed oil lamps, each of six concentric wicks; and of two drums of lenses of 920 mm. focal distance, twelve lenses in each drum. The optical apparatus is specially designed on the system of Dr. John Hopkinson, F.R.S. for a double flashing light, and shows two flashes in quick succession, at intervals of half a minute. Attention has of late been directed to the subject of superposed lights in lighthouses, which became a necessity when several small luminaries had to be substituted for the large coal, or wood, fire of our early lighthouses. The credit of first superposing lighthouse luminaries is doubtless due to Smeaton, who lighted his lantern in 1759 with 24 large tallow candles in two tiers. The idea was followed in 1790 with the first revolving light, established at the St. Agnes lighthouse, Scilly Islands, which consisted of 15 oil lamps and reflectors, arranged in three groups, and in three tiers. The number of the lamps and reflectors at this and other first-class lights, was afterwards extended to 30, and in four tiers. In 1859 Mr. J. W. D. Brown, of Lewisham, proposed superposed lenses for signal and lighthouse lanterns, with a separate light for each tier of lenses. In 1872 Mr. John Wigham, of Dublin, proposed superposed lenses for lighthouses, in conjunction with his large gas flames, and the first application of these was made in 1877, at the Galley Head lighthouse, County Cork. In 1876 Messrs. Lepaute and Sons, the eminent lighthouse optical engineers of Paris, made successful experiments with superposed lenses and mineral oil flames, and one of their apparatus was exhibited at the Paris International Exhibition of 1878. The results of these experiments were given by M. Henry Lepaute, in a paper contributed to the Congress at Havre, in 1877, of the French Association for the Advancement of Science. The Eddy-

stone represents the first practical application of superposed lenses of the first order, with oil as the illuminant.

The apparatus at the Eddystone is provided with two six wick burners of the Trinity House improved type, and has a minimum intensity for clear weather of about 38,000 candle units, and a maximum intensity of about 160,000 candle units for atmosphere impaired for the transmission of light. The chandelier light in Smeaton's lantern was unaided by optical apparatus. I have found by experiment that the aggregate intensity of the beam from the 24 candles was 67 candle units nearly. The maximum intensity of the flashes now sent to the mariner is about 2380 times that of the candle beam, while the annual cost for the mineral oil illuminant is about 82 per cent. less. The sound signal for foggy weather consists of two bells of 40 cwt. each, mounted on the lantern gallery, and rung by machinery. If any wind occurs with the fog, the windward bell is sounded. The distinctive character of the signal is two sounds of the bell in quick succession every half minute, thus corresponding with the character of the light signal.

The tendency of the curvilinear outline near the base of Smeaton's and of other similar sea towers that have followed it, to elevate the centre of force of heavy waves on the structure, induced me to adopt a cylindrical base for the new lighthouse, which is found to retard the rise of waves on the structure, while it affords a convenient platform for the light-keepers, and adds very considerably to their opportunities for landing and relief. The Town Council and inhabitants of Plymouth having expressed a desire that Smeaton's lighthouse should be re-erected on Plymouth Hoe, in lieu of the Trinity House sea mark thereat, the Trinity House, who, as custodians of public money, had no funds available for such a purpose, undertook to deliver to the authorities at Plymouth, at actual cost for labour, the lanterns and the four rooms of the tower. These have been re-erected by public subscription, on a foundation of granite, corresponding nearly with the lower portions of Smeaton's tower, and it is to be hoped that it will be preserved by the town of Plymouth as a monument to the genius of Smeaton, and in commemoration of one of the most successful and beneficent works in civil engineering.

It is extremely difficult to estimate with a fair degree of accuracy the maximum force of the waves with which some of the most exposed of these sea structures may occasionally have to contend. The late eminent lighthouse engineer, Mr. Thomas Stevenson, carried out a long series of experiments with a self-registering instrument he devised for determining the force of sea waves on exposed structures. He found at the Skerryvore rock lighthouse the Atlantic waves there gave an average force for five of the summer months, in 1843–4, of 611 lbs. per square foot. The average result for the six winter months of the same year was 2086 lbs. per square foot, or three times as great as in the summer months. The greatest force registered was on the 29th March, 1845, during a westerly gale, when a pressure

of 6083 lbs., or $2\frac{3}{4}$ tons nearly, per square foot was recorded. After Smeaton had carefully considered the great defect of the building of Rudyerd at the Eddystone, viz. want of weight, he reported that, "if the lighthouse was to be so contrived as not to give way to the sea, it must be made so strong as that the sea must be compelled to give way to the building." Smeaton also had regard to durability as an important element in the structure, for he adds, "in contemplating the use and benefit of such a structure as this, my ideas of what its duration and continued existence ought to be, were not confined within the boundary of one age or two, but extended themselves to look towards a possible perpetuity." Thus Smeaton soon arrived at the firm conviction that his lighthouse must be built of granite, and of this material nearly all lighthouses on exposed tidal rocks have since been constructed, while those on submerged sandbanks are open structures of iron, erected on screw piles or iron cylinders. The screw pile was the invention of the late Mr. Alexander Mitchell, of Belfast.

We have here a model of the first lighthouse, erected in 1838 on these screw piles, at the Maplin Sands, on the north side of the estuary of the Thames; under the direction of the late James Walker, F.R.S. then Engineer-in-Chief to the Trinity House. A lighthouse on the principle of minimum surface exposed to the force of the waves, of which we have here a model, was erected on the chief rock of the dangerous group of the Smalls, situated about eighteen-and-a-half miles off Milford Haven, by Mr. John Phillips, a merchant and shipowner of Liverpool.

The work was designed and erected under great difficulties by Mr. Henry Whiteside, a native of Liverpool, and a man of great mechanical skill and undaunted courage. Added to his mechanical ability, Whiteside possessed a great love and knowledge of music, and had, previous to the erection of his lighthouse, excelled in the construction of violins, spinnettes, and upright harpsichords. The lighthouse, commenced in 1772, was intended to be erected on eight cast iron pillars, sunk deep into the rock. This material was, however, soon abandoned for English oak, as being more elastic and trustworthy. The work was completed and lighted in 1776, with eight lamps and glass facetted reflectors, similar to the one before us.

In 1817, sixteen improved lamps and silvered paraboloidal reflectors were substituted for these; and the lighthouse, although sorely tried by winter storms, was (with the aid of yearly repairs and strengthening) enabled to send forth its beneficent beam until the year 1856, when the Trinity House commenced the erection of a lighthouse of granite, as shown by this model. The vibrations of the old wooden structure must have been very considerable with heavy storms, for the lightkeepers occasionally found it sufficient to cause a bucket of water, placed in the living room to spill just half its contents. It was in this lighthouse that the painful circumstance occurred in the year 1802, of the death of one of the lightkeepers.

In those days only two men inhabited the lighthouse at a time; one of them was taken ill, and the means employed by his companion for obtaining relief proved ineffectual. He hoisted a signal of distress, but owing to stormy weather no landing could be effected, and after many days of extreme suffering, the poor fellow, named Thomas Griffiths, breathed his last, when the survivor, Thomas Howell, fully realised the awful responsibilities of his position; decomposition would quickly follow, and the atmosphere of the small apartment would be vitiated. The body could not be committed to the sea, as suspicion of murder would probably follow. Howell was a cooper by trade, and he was thus enabled to make a coffin for his dead companion, out of boards obtained from a partition in the apartment. After very great exertion the body was carried to the outer gallery, and there securely lashed to the railing. For three long weeks it occupied this position before the weather moderated, yet night after night Howell faithfully kept his lights brightly burning. When a landing was at last effected, his attenuated form demonstrated the sufferings, both mental and physical, he had undergone; indeed, several of his friends failed to recognise him on his return to his home. Since this sad occurrence the Trinity House have always maintained three lightkeepers at their isolated rock stations. The present lighthouse was designed by the late Engineer-in-Chief of the Trinity House, Mr. James Walker, F.R.S. and I had the honour of executing the work as resident engineer. The foundation stone was laid on the 26th June, 1857, and the light was exhibited on the 7th August, 1861. The work was completed by the Trinity House, at a cost of 50,125*l.*, being about twenty-four per cent. under the lowest amount at which it had been ascertained that it could have been executed by contract.

Probably the most exposed rock lighthouse is that on the Bishop (the westernmost of the rocks of Scilly), shown on the diagram. Its position is doubtless one of the most important to mariners, warning them as it does of the terrible dangers where, on the 22nd of October, 1707, Sir Cloudesley Shovel, with the *Association*, *Eagle*, and *Romney*, were lost, with about 2000 men. The Bishop is also the guiding light for the entrances to the English and Bristol Channels. The rock, composed of a very hard pink-coloured granite, is about 153 feet long by 52 feet wide at the level of low water of spring tides. It stands in over twenty fathoms water, is steep-to all round, and is exposed to the full fury of the Atlantic. It was at first feared that the width of the rock was not sufficient for the base of a stone tower of adequate dimensions to withstand the heavy wave shocks it would have to resist, and an open structure of wrought and cast iron (shown on the diagram) was determined on. The work was jointly designed by the late Engineer-in-Chief to the Trinity House, and my father, the Superintending Engineer, who afterwards erected the structure, at which I had the honour of acting as Assistant Engineer.

The work was commenced in 1847, and at the end of the working

season of 1850, the lighthouse was so far completed as to be in readiness for receiving the lantern and the illuminating apparatus; and it was left with confidence, to resist the storms of the approaching winter. But during a very violent storm, between 11 p.m. of the 5th and 3 a.m. on the 6th of the following February, the lighthouse was completely destroyed and swept from the rock. On further consideration of the matter, the Trinity House determined, on the recommendation of their engineers, to proceed with a stone structure, and my father was appointed to build the lighthouse, I acting as before as Assistant Engineer. The work was proceeded with in the spring of 1851. In order to obtain the greatest possible diameter of base for the tower that the rock would admit of, it was found necessary to lay a portion of the foundation on the most exposed side of the rock, at the level of one foot below low-water of spring tides; and, although every possible human effort was made by the leader, and his devoted band of workers, the foundations were not completed until the end of the season of 1852. Soon after this, my brother, Mr. William Douglass, now Engineer-in-Chief to the Commissioners of Irish Lights, succeeded me as assistant engineer at the work. The lighthouse was completed in 1858, and its dioptric fixed oil light of the first order was first exhibited on the 1st of September of that year. Soon afterwards, its exposure to heavy seas during storms, was fully realised. On one occasion the Fog Bell was torn from its bracket at the lantern gallery at 100 feet above high water, and the flag staff with a ladder, which were lashed outside the lantern, were washed away. The tremor of the tower on these occasions was such as to throw articles off shelves, and several of the large glass prisms of the Dioptric Apparatus were fractured. After some time it was found that several of the external blocks of granite situated a few feet above high water were fractured by the excessive strains on the building. In 1874 the tower was strengthened from top to bottom by heavy iron ties, bolted to the internal surface of the walls; but, after a violent storm in the winter of 1881, there was evidence of further excessive straining at the face of the lower external blocks of masonry, when the Trinity House, on the advice of their engineer, determined on the re-erection of the lighthouse. This was accomplished, as shown on the diagram, by encasing the existing tower with carefully dovetailed granite masonry, each alternate block of the new granite being dovetailed to the old. The work was one of considerable difficulty, owing to the necessity for maintaining the light throughout the progress; and the risk to the workmen was great, especially at the upper part of the old tower, owing to the narrow ledge on which the work had to be executed. I am, however, thankful to state that the new Lighthouse has been successfully completed by my son, Mr. W. T. Douglass, who was also my assistant engineer at the Eddystone; and with the same complete immunity from loss of life, or limb, to any person employed, as with the two previous structures on this rock. The optical apparatus consists of two superposed tiers of lenses of the type adopted at the Eddystone,

but of larger dimensions, as suggested by the late Mr. Thomas Stevenson, for obtaining greater efficiency with the larger flamed luminaries recently adopted. The apparatus is provided with two Trinity House improved mineral oil burners, and has a minimum intensity for clear weather of about 80,000 candle units, and a maximum intensity for thick weather of about 513,000 candle units. The character of the light is Double Flashing, showing two flashes, each of four seconds duration, in quick succession at periods of one minute. The flashes of this light, and those of a light lately completed at about eight nautical miles from it, on Round Island, are the most intense yet attained with oil flames for beacon lights; and it may be stated that, with no other illuminant at present known to science, could these results be carried out within the space available at the Bishop rock, and under the circumstances attending that work. The fog signal recently adopted at this station, in lieu of the bell, is by the electrical explosion of four-ounce charges of gun-cotton, at intervals of five minutes. The apparatus provided for this form of fog signal is shown on the diagram. It consists of a wrought-iron crane (attached to the lantern) which is raised and lowered by a worm wheel and pinion. When the crane is lowered, its end reaches near the gallery, where the lightkeeper suspends the charge of gun-cotton, with its detonator attached, to the electric cable, which is carried along the crane and through the roof of the lantern to a dynamo electric firing machine. After suspending the charge, the jib of the crane is raised to its upper position, when the charge is fired nearly vertically over the glazing of the lantern, and thus without causing damage to it.

The large and heavy optical apparatus is rotated automatically by compressed air, which is stored in two vertical steel reservoirs, fixed at the centre of the tower. The air is compressed by a small Davey Safety Motor. A winch, worked by the compressed air, is fixed on the lantern gallery for landing the lightkeepers, stores, &c.

The numerous outlying shoals surrounding the shores of this country, particularly off the east coast, were an early cause of anxiety to those responsible for the guidance of mariners. And in addition to buoys as sea marks by day, floating lights, as guides by night, were found to be a necessity. The first light-vessel was moored at the Nore Sand in 1732, and another near the Dudgeon Shoal in 1736. We have here a model of the latter vessel, from which we may judge of the pluck and hardihood of the crews who manned them; especially when we remember that there were no chain cables in those days; the vessel having to be moored with a cable of hemp, which, owing to the constant chafing, occasionally parted during winter storms, when, to save their lives, the crew had to put out another anchor if possible, or set such storm canvas as they could, to keep her off a lee shore, and endeavour to reach a place of safety. The illuminating apparatus of these vessels consisted of a small lantern, and

flat wick oil lamps, fixed at a yard arm, and here appears to have occurred the first application of a distinctive character to beacon lights, for the Dudgeon was fitted with two lights, one being placed at each arm of the yard. The next light-vessel was placed at the Newarp Shoal in 1790, and in 1795 one was placed at the north end of the Goodwin Sands. The two latter vessels were provided with three fixed lights, and the lanterns were larger and surrounded each mast-head, as shown by the model before us. An improvement was also effected in these lights by providing each lamp with a silvered reflector.

In 1807 the late Mr. Robert Stevenson, the engineer of the Bell Rock lighthouse, to whom and his successor is due much valuable engineering and optical work connected with coast lighting, designed a larger lantern to surround the mast, and capable of being lowered to the deck for properly trimming the lamps. Soon after the adoption of the system of catoptric illuminations in lighthouses, it was extended to floating lights; each lamp and reflector was hung in gimballs, to ensure horizontal direction of the beams of light during the pitching and rolling of the vessel. We have here one of these apparatus. The intensity of the beam sent from it was 500 candle units nearly.

On the 1st January, 1837, the Trinity House installed the first revolving floating light, at the Swin Middle, and, later in the same year, another on board the Gull light-vessel. The lamps and reflectors were carried on a roller frame surrounding the mast, and rotated through light shafting, by clockwork placed between decks. There were nine lamps and reflectors arranged in three groups, of three each, and thus the collective intensity of each flash was equal to that of three fixed lights, or 1500 candle units nearly. In 1872 the Trinity House further increased the dimensions of the lanterns and reflectors of their floating lights, the lanterns from six to eight feet in diameter, with cylindrical instead of polygonal glazing, and the reflectors from 12 inches to 21 inches diameter at the aperture. These improvements, together with the adoption of improved burners, have effected a considerable increase in the intensity of these lights; and, during the last two years, a further improvement has been obtained, by the adoption of concentric wick burners with more condensed flames, and of higher illuminating power, by which the intensity of the beam from each reflector has been raised to 5000 candle units; being just ten times the intensity of the smaller apparatus; while, by the adoption of mineral oil in lieu of colza, the annual cost for the illuminant has been reduced 51 per cent.

Dioptric apparatus for light-vessels was proposed by M. Letourneau in 1851, several small fixed light apparatus being intended to be employed in each lantern, and arranged nearly in the same way as the reflectors. This arrangement has been adopted in some instances by Messrs. D. and T. Stevenson, engineers to the Commissioners of Northern Lighthouses, and by the engineers of the

French Lighthouse service; but, for efficiency, and adaptability to meet the rough duty to which floating lights are occasionally subjected, in stormy weather and collisions, this system has been found to be inferior for this service to the catoptric.

An interesting experiment was recently made by the Mersey Docks and Harbour Board with the electric arc light, on board one of their light-vessels at the entrance of the Mersey, but unfortunately it did not prove successful. The present difficulties experienced afloat with this powerful illuminant will doubtless be overcome, and it will be found to be, as in lighthouses, by far the most efficient illuminant for some special stations, where a higher intensity than can be obtained with flame luminaries is demanded. Experiments have been in progress during the past two years at the "sunk" light-vessel, off the coast of Essex, for maintaining electrical telegraphic communication with the shore for reporting wrecks and casualties in the locality. This vessel is connected with the Post Office at Walton-on-Naze, through nine miles of cable. The instruments adopted are the Wheatstone A.B.C. "Morse," and the Gower Bell telephone—the telephone for the first time for this purpose on board a vessel at sea, and its efficiency has been found to be so perfect, that it is preferred by the operators to the telegraphic instruments. Many difficulties have been experienced in maintaining reliable communication during stormy weather, owing to consequent wear and tear of the connections with the vessel, but the system, which was designed and carried out by the Telegraphic Construction and Maintenance Company, is now working satisfactorily. Unfortunately, however, it is found to be too costly for adoption, except in very special cases.

In 1876, Mr. Julius Pintsch, of Berlin, patented in this country his system of illuminating buoys or other floating bodies by compressed oil gas, and in 1878 one of these buoys was experimentally tried at sea with success by the Trinity House. The system is similar to that previously adopted by Mr. Pintsch with great success in the lighting of railway carriages, but with the addition for buoys of a specially constructed lantern, containing a small cylindrical lens for a fixed light. Through the kindness of the Pintsch's Lighting Company we have here one of these apparatus, producing an intensity in the beam of about twenty candle units. With the charge of gas contained in the buoy the light is shown continuously, night and day, from two to four months, according to the dimensions of the buoy, without re-filling or requiring any other attention, except occasional cleaning of the lens and the glazing of the lantern. In 1883, Mr. William B. Rickman patented a very ingenious addition to this apparatus for producing occulting or flashing light. The apparatus is automatically worked by the issuing compressed gas on its way from the buoy to the burner. After passing the regulator, where the pressure of the gas is reduced for burning, it enters a cylindrical chamber, covered with a diaphragm of very flexible specially prepared leather; this diaphragm, on being slightly raised by the inflowing

gas, communicates motion to a lever, which, assisted by a spiral spring, closes the inlet pipe, and opens at the same time the passage to the burner. As the gas passes on and is consumed at the burner the diaphragm, by its own weight, assisted by the springs, sinks, and, touching the lever, closes the outlet aperture to the burner, and, at the same moment, opens the inlet of the gas from the buoy for another charge. Thus the light is extinguished while the gas is entering the chamber and until the latter is re-filled, when the passage from the buoy is again closed by the rising of the diaphragm. A small pilot jet is constantly burning to ensure the re-ignition of the gas when re-admitted to the burner. It is evident that several characteristic distinctions of light may be obtained by modifications of this ingenious apparatus. About 150 buoys lighted on the Pintsch system are already rendering valuable service to mariners in various parts of the world. For the more important stations at sea, where light-vessels are now employed, the system is considered to be yet wanting in that trustworthiness which should be the leading characteristic of all coast lighting. Very important experiments have lately been made by the Lighthouse Board of the United States, at their general depôt at Tompkinsville, New York, with buoys lighted electrically by glow lamps, operated through submarine conductors from the shore. These experiments have proved so successful that an installation for marking the Gedney's Channel, entrance of Lower Bay, New York Harbour, with six buoys and 100 candle glow lamps, was lighted on the 7th of November last. Gas buoys were considered inapplicable for this special case, owing to their form and size rendering them liable to break adrift, particularly when struck by floating ice or passing vessels. The buoy adopted for the service consists of a spar 46 feet long, having its lower end shackled direct to a heavy iron sinker, resting on the bottom. At the upper end the buoy is fitted with an iron cage, enclosing a heavy glass jar, in which is placed the glow lamp of 100 candle units intensity. The cable is secured by wire staples, in a deep groove cut in the buoy, and covered by a strip of wood. For a distance of several feet at the lower end of the buoy the cable is closely served with iron wire, over which is wound spun yarn, to prevent injury from chafing on the shackle and sinker. The central station on shore, with steam engines and dynamos in duplicate, is on Sandy Hook, at a distance from the extreme buoys of about three nautical miles. The installation is reported to be working continuously and successfully. For auxiliary or port lights on shore, where no collisions can occur, the Pintsch gas system is found to be very perfect. At Broadness, on the Thames, near Gravesend, the Trinity House erected, in 1855, an automatic lighthouse illuminated on Pintsch's system, as shown by the diagram. This small lighthouse shows a single flashing light, at periods of ten seconds, the flashes having an intensity of 500 candle units. The flashes and eclipses are produced with perfect regularity by special clockwork, which also turns on the gas supply to the burner at

sunset and off again at sunrise. It is also arranged for periodic adjustment, for the lengthening and shortening of the nights throughout the year. This automatic light is in the charge of a boatman, who visits it once a week, when he cleans and adjusts the apparatus, and cleans the glazing of the lantern. An automatic lighthouse similar to that at Broadness has been lately installed at Sunderland by the River Wear Commissioners, on a pier which is inaccessible in stormy weather. In 1881–82 several beacons automatically lighted by petroleum spirit, on the system of Herr Lindberg and Herr Lyth, of Stockholm, were established by the Swedish Lighthouse authorities, and are reported to be working efficiently. In 1885, a beacon or automatic lighthouse on this system was installed by the Trinity House on the Thames, near Gravesend, and has been found to work efficiently. The light is occulting at periods of about two seconds; the occultations are produced by an opaque screen rotated around the light, by the ascending currents of heated air from the lamp acting on a horizontal fan. As there is no governor to the apparatus the periods of the occultations are subject to slight errors compared with those of the gas light controlled by clockwork. In 1844 an iron beacon, lighted by a glow lamp and the current from a secondary battery, was erected on a tidal rock near Cadiz. Contact is made and broken by a small clock, which runs for 28 days, and causes the light to flash for five seconds at periods of half a minute. The clock is also arranged for eclipsing the light between sunrise and sunset. The apparatus is the invention of Don Isas Lavoden, of Cadiz, to whom I am indebted for kindly showing me the light in action when on a visit to Cadiz in 1885. There is every probability that automatic beacons, lighted either by electricity, gas, or petroleum spirit, will, in consequence of their economy in maintenance, be extensively adopted in the future.

Coal and wood fires, the flames produced by the combustion of tallow, nearly all the animal, vegetable, and mineral oils, coal and oil gas, and the lime light, have been employed from time to time in lighthouse illumination, and last but not least, the electric light. None of these illuminants have received such universal application in all positions both ashore and afloat as mineral oil at the present moment, and justly so, when we consider its efficiency and economy for the purpose. So recently as 1822, the last beacon coal fire in this country was replaced by a catoptric oil light, at Saint Bees lighthouse, on the coast of Cumberland. We have here diagrams of two of these coal fire beacons, one of them designed and erected by Smeaton in 1767 on his lighthouse at the Spurn Point, on the east side of the entrance to the Humber. So late as 1845 sperm oil was entirely used in the lighthouses and light-vessels of the Trinity House; but, shortly afterwards, colza was adopted with the same efficiency, and with a saving in annual cost of about 44 per cent. In 1861, experiments were made by the Trinity House for determining the relative efficiency and economy of colza and mineral oil for lighthouse illumi-

nation; but owing to the imperfect refinement of the best samples of the latter then procurable in the market, together with its high price, the result of the investigation was not so satisfactory as to justify a change from colza. In 1869 the price of mineral oil of good illuminating quality and safe flashing point, was found to be procurable at about half the price of colza, when the Trinity House determined to make a further series of experiments, and by these it was ascertained that, with a few simple modifications of the Argand burners then in use, they were rendered very efficient for the purpose, it was also found that these burners were thus considerably improved for the combustion of colza. A change from colza to mineral oil was then commenced, and mineral oil is now generally adopted in the lighthouses and light-vessels of the Trinity House service, and with even greater economy than was at first anticipated; the price of this illuminant being now rather less than one-third that of colza. The most powerful oil burner then in use was one of four concentric wicks, the joint production of Arago and Fresnel, and adopted by the French lighthouse authorities about the year 1825, in conjunction with the then new dioptric system of optical apparatus of Fresnel. The standard intensity of the combined flames of this burner, one of which we have here, was 260 candle units. A further development was made, during the experiments of the Trinity House in 1871, by increasing the number of wicks from four to six, which more than doubled the intensity of the light, while effecting a condensation of the luminary per unit of focal area, or in other words improved the optical efficiency 70 per cent. We have here also one of these burners. I have since devised an Argand burner for the combustion of all illuminating gases and oils, whereby still further condensation of the flames, together with greater intensity and economy of combustion, is obtained, and the glass chimney is protected from breakage. These improvements are effected by a special arrangement and distribution of the air currents through the rings of flame, and between them and the glass chimney. (See Models.) We are thus enabled on this system to increase the dimensions of lighthouse burners, for gas and oil, for ten or more rings of flame. With ten rings we obtain an aggregate intensity, when burning cannel gas and good mineral oil, of considerably over 2000 candle units, while the improved efficiency of the luminary for optical condensation of the radiant light, per unit of focal area, as compared with the luminary of our Fresnel four-wick oil burner, has been in each case increased 109 per cent. With reference to the perfect combustion of these highly condensed flames I may state that the efficiency for gas is exactly double that of the London standard Argand burner, viz. when consuming gas of the London standard of 16 candles, the light produced is at the rate of 6·4, instead of 3·2, candles per cubic foot. In addition to a single ring gas burner of this type we have two burners of ten rings of flame, and models of their flames, one for gas and the other for mineral oil. These burners are all of the Trinity House new pattern, both

gas and oil, and they are of the same general arrangement for combustion, except that the oil burner is provided with cotton wicks. Both produce flames of nearly the same form, dimensions, intensity, and colour.

The first application of coal gas to lighthouse illumination was made at the Troon lighthouse, Ayrshire, in 1827; and in 1847 it was adopted at the Hartlepool lighthouse, Durham; when for the first time it was employed in combination with dioptric apparatus of the first order of Fresnel. The slow progress made with coal gas in lighthouses, except for harbour lights, where the gas could be obtained in their vicinity, as at Hartlepool, was chiefly due to the great cost incurred in the manufacture of the small quantity required, and at the usual isolated positions occupied by coast lighthouses, involving extra cost both for labour and for the extra transport of the coal. In 1865 the attention of lighthouse authorities was directed to gas as an illuminant for lighthouses by Mr. John R. Wigham, of Dublin, whose system was tried in that year at the Howth Bailey lighthouse, Dublin Bay. The gas burner of Mr. Wigham, one of which we have here, consists of seven concentric rings, of single flat-flame burners, amounting in the aggregate to 108. The burner is used without a glass chimney, and thus there is no appreciable condensation of the group of flames for their employment at the focus of optical apparatus, and the relative aggregate intensity of the seven rings of flat flames per unit of focal area, as compared with the four concentric flames of the old four-wick oil burner of Fresnel, are only $2\frac{1}{2}$ per cent. higher than the latter. The burner has five powers for varying states of the atmosphere. For the minimum intensity 28 jets are employed, and with the whole 108 jets there is a maximum aggregate intensity of the flames, with cannel gas, of about 2500 candle units. Several lighthouses on the coast of Ireland have been illuminated with gas on the system of Mr. Wigham, and two at Haisboro, on the coast of Norfolk. In 1878 Mr. Wigham installed at the Galley Head lighthouse, County Cork, his system of superposed gas flames and group flashing light, which consisted of four of his large gas burners vertically superposed. In conjunction with these were four tiers of first order annular lenses, eight in each tier. By successive lowering and raising of the gas flames at the focus of each tier of lenses, he produced his group flashing distinction. This light shows, at periods of one minute, instead of the usual single flash from each lens, or vertical group of lenses, a group of short flashes, varying in number, between six and seven. The unavoidable uncertainty with this system in the number of flashes contained in each group is unfortunate for the mariner, who, with the continued increase in the number of coast lights, requires the utmost precision in the distinctive character adopted for each.

In 1857 an experimental trial of the first magneto-electric machine of Holmes, for the practical application of the electric light, was made

by the Trinity House at Blackwall, under the direction, and to the great delight, of their scientific adviser, Faraday; and after a series of experiments, the satisfactory report of Faraday encouraged the Trinity House to order a practical trial of a pair of the Holmes machines. The trial was made at the South Foreland high lighthouse, by Faraday and Holmes, on the 8th of December, 1858, when electricity was found to be a formidable rival to oil and gas for lighthouse illumination, and this position it maintains to the present day. The trials of this arc light were made at the focus of the first order dioptric apparatus for oil light, which was very imperfect for the purpose, but they were sufficiently encouraging to lead the Trinity House, under the advice of Faraday, to proceed further with the electric light for lighthouses. Faraday thus wrote in his report to the Trinity House: "I beg to state that, in my opinion, Professor Holmes has practically established the fitness and sufficiency of the magneto-electric light for lighthouse purposes, so far as its nature and management are concerned. The light produced is powerful beyond any other that I have yet seen so applied, and in principle may be accumulated to any degree; its regularity in the lantern is great, its management easy, and its care there may be confided to attentive keepers of the ordinary intellect and knowledge." These truly prophetic words of Faraday have been entirely realised; electricity still stands foremost in the illumination of our coasts, and appears destined to be one of the greatest blessings ever conferred on humanity, and more especially on "those who go down to the sea in ships." On the 1st of February, 1862, Holmes's machines and apparatus for the electric light were installed at Dungeness lighthouse, and in 1863 the French lighthouse authorities followed, by an installation of the Alliance Company's magneto-electric machines and apparatus for fixed lights, at each of the two lighthouses at Cape La Hève. We have here the first dioptric apparatus designed and manufactured by Messrs. Chance Bros. & Co. of Birmingham, for the electric fixed light at Dungeness. We have also one of the Holmes lamps employed there. The lamp used at the previous experiments was devised by M. Duboscq, of Paris. This lamp of Holmes is similar to those of Duboscq and Serrin, excepting that the upper and lower carbons and holders are balanced and regulated through pulleys and small catgut cords, instead of by rack and pinions. The carbons are $\frac{1}{4}$-inch square, and the mean intensity of the light in the arc was 670 candle units nearly. We have here samples of the carbons employed from time to time in the development of the electric light in lighthouses; we have also a Bergot lamp, fitted with the fluted form of carbons I have recently devised. They are of the dimensions now in use at the Saint Catherine's lighthouse, and are giving a mean intensity in the arc of 40,000 candle units. Cylindrical compressed carbons were soon manufactured for the electric light, and were found to be more homogeneous in quality and the flickering of the light less than with the original square carbons, which were simply sawn from the residual carbon of gas retorts; but

there was still the objectionable crater at the points, whether direct or alternating currents were employed, involving flickering from the incessant shifting of position at the points. A considerable loss of radiant light was also involved, particularly when condensing it optically. The flickering was somewhat reduced by an improvement of Messrs. Siemens, in providing the carbons with a graphite core, but with the increasing powers of currents and in the necessary dimensions of carbons the results were far from satisfactory. With the fluted form of carbon shown on the diagram the formation of the crater is prevented, and the arc is held centrally at the points of the carbons; there is thus, in addition to comparatively steady light, nearly uniform radiation in azimuth, and over a greater vertical angle for optical condensation. It now appears to me, after some practical experience with this form of carbon, that it is impossible to determine a practical limit to the dimensions of carbons that may be efficiently employed. With carbons of the actual size shown on the diagram an intensity of about a million candle units should be produced in the arc, and about 150 millions of candle units in the condensed flashes from the optical apparatus of the dimensions now employed for oil and gas flames in lighthouses. Such an intensity is about 400 times that possible at the focus of such apparatus with a flame luminary. Such results as these were probably in the mind of Faraday when he reported that "in principle this light may be accumulated to any degree." Flashes of the great intensity here referred to could only be employed in atmosphere impaired for the transmission of light. In clear weather they would be found to be far too dazzling to the eyes of the mariner, while an intensity of about 50,000 candle units is found to be sufficient for his guidance, and in thick fog no possible intensity can be of practical value for navigation. There are, however, various gradations of impaired atmosphere, between clear weather and thick fog, in which the highest available intensity is doubtless desirable at many important landfall stations for obtaining the greatest possible range of visibility. On the other hand, at the majority of stations in narrow waters, the maximum intensity now obtained with flame light is found to be more generally efficient for navigation than higher intensities.

In 1881 the question of the relative merits of the three lighthouse illuminants—electricity, gas, and mineral oil—was receiving the attention of the lighthouse authorities of this country, which resulted in the Trinity House accepting the responsibility of carrying out an investigation at the South Foreland, of universal importance to the mariner. In the photometrical and electrical portions of this work the Trinity House were aided by the labours of Professor Harold Dixon, F.R.S. and Professor W. Grylls Adams, F.R.S. which contributed very largely to the success of the investigation. The experiments were carried on during a period of over twelve months, and a vast amount of very valuable evidence was collected from numerous

observers, trained and untrained, scientific and practical. The report of the Committee was presented to both Houses of Parliament, by command of Her Majesty, in 1885. The final conclusions of the Committee are given in the following words: "That for the ordinary necessities of lighthouse illumination, mineral oil is the most suitable and economical illuminant, and that for salient headlands, important landfalls, and places where a very powerful light is required, electricity offers the greatest advantages."

I have already referred to the necessity, with the present development of maritime commerce, that every beacon light maintain a clearly distinctive character. When the optically unaided flames of coal fires were the illuminants of our lighthouses, distinctive characters, owing to the small number of lights then employed, were of little importance, and the only distinctions then possible were the costly ones of single, double, or triple lighthouses at one station; but with the enormous increase that has since occurred in the floating commerce of the world, and with the necessary laws now in operation requiring all vessels to carry lights, trustworthy individuality in coast beacon lights has become a positive necessity. Until very recently the distinctive characters consisted of the following: viz.—fixed white, fixed red, revolving white, revolving red, and revolving white and red alternately. The revolving lights showed a flash at periods of 10 seconds, 20 seconds, 30 seconds, one minute, 2 minutes, 3 minutes, and 4 minutes. There were also intermittent or occulting lights, having an eclipse at periods of half-a-minute, one minute, or 2 minutes. It is now generally considered that fixed lights are no longer trustworthy coast signals, owing to their liability to confusion with other lights, both ashore and afloat. It is also considered that in these days of high speed vessels the period of the character of a coast light should not if possible exceed half-a-minute. The revolving or flashing class of lights are probably the most valuable, on account of their superior intensity, as compared with the fixed or occulting class, the light during the intervals of eclipse being condensed into each succeeding flash by the revolving lenses or reflectors, and thus, with the same expenditure of the illuminant, an intensity is obtained in the flashes of five to eight times that of the fixed or occulting class. Where local dangers are required to be guarded by coloured sectors of danger light with well-defined limits, this can only be accomplished with the fixed or occulting class of lights. We will illustrate this with the model before us. We will also show the clear difference of character, not generally realised, between flashing and occulting lights. A system of occulting lights for lighthouses was proposed by the late Charles Babbage, F.R.S. in 1857; but as it excluded the flashing or most powerful of the existing lights, it did not receive much favour from lighthouse authorities. And in 1872, 'Distinctive Characters for Coast Lights,' was the subject of a paper by Sir William Thomson, F.R.S. at the Brighton Meeting

of the British Association for the Advancement of Science, when he directed attention to the extreme importance of ready identification of lights at sea, and proposed the use of quick-flashing lights, their flashes being of longer or shorter duration; the short and long flashes representing the dot and dash of the Morse alphabet as used in telegraphy. It was found, however, that the number of symbols in one alphabetical code would not be sufficient, on a thickly lighted coast, to ensure individuality, and render each distinction perfectly trustworthy. Further, that very rapid repetition of each symbol is not required by the mariner, and would involve loss of accumulative power in the flashes, besides incurring unnecessary wear and tear in rotating heavy optical apparatus. Yet much is to be done in the direction of simple distinction. At the Montreal Meeting of the British Association, in 1884, I submitted a paper on 'Improvements in Coast Signals,' in which were suggested two alphabetical codes of flashing lights, and one of occulting, all having the same period of the symbol, viz. half-a-minute. In one of the codes of flashing lights long and short flashes were proposed, as previously by Sir William Thomson, and, in the other, there were proposed white and red flashes. In the occulting series, long and short eclipses were proposed to be substituted for the long and short or white and red flashes of the flashing codes. The system has the advantage of application to all existing lighthouse apparatus, and many lights have been altered to selected symbols of each of these series.

Little was ever accomplished in the way of warning or guidance to the mariner during fog, until about the middle of this century. Previously, a few bells had been established at lighthouses in this country and abroad, and gongs of Chinese manufacture had been in general use on board our light-vessels, but both instruments are now acknowledged to be wanting in the efficiency now demanded in fog, to meet the requirements of navigation. The first important improvement in fog signals, for the service of mariners, was made by the late Mr. Daboll in 1851, who submitted to the United States Lighthouse Board, in that year, a powerful trumpet, sounded by air, compressed by horse-power. The apparatus was installed at Beaver Tail Point, Rhode Island, and the favourable results obtained with it stimulated Mr. Daboll, under the encouragement of the United States Lighthouse Authorities, to the further development of the apparatus; and ultimately, he employed Ericsson's Caloric Engine, as the motive power, with automatic gearing for regulating the blasts. In 1854, some experiments on different means of producing sounds for coast signals were made by the engineers of the French Lighthouse Department, and in 1861–2, MM. Le Gros and Saint Ange Allard, of the Corps des Ponts et Chaussées, conducted a series of experiments upon the sound of bells, and the various methods of striking them. In 1862, Mr. Daboll submitted his improved fog trumpet apparatus of about three horse-power in the blasts, to the Trinity House, who, under the

advice of Faraday, made experimental trials with it in London, and afterwards gave it a practical trial at the Dungeness lighthouse, where experiments were made with it against bells, guns, and a reed fog horn of Professor Holmes, whose services have been already referred to in connection with the first practical application of the electric light. This fog horn of Holmes was sounded by steam, direct from one of the boilers employed at the station for his electric light. The results of these experiments were in favour of Daboll's trumpet, and in 1869, one of these instruments was installed on board the Newarp light-vessel. In the same year, Holmes, having effected further improvements with his steam horn, his apparatus was fitted on board two light-vessels and sent out to the coast of China, where they were found to give great satisfaction, as compared with gong signals. In 1863 a committee of the British Association for the Advancement of Science memorialised the President of the Board of Trade, with the view of inducing him to institute a series of experiments upon fog signals. The memorial, after briefly setting forth a statement of the nature and importance of the subject, described what was then known respecting it, and several suggestions were made relative to the nature of the experiments recommended. The proposal does not appear to have been favourably entertained by the authorities to whom it was referred, and the experiments were not carried out. In 1864 a series of experiments was undertaken by a commission appointed by the Lighthouse Board of the United States to determine the relative powers of various fog signals which were submitted to the notice of the Board. In 1872, a committee of the Trinity House, with the object of ascertaining the actual efficiency of various fog signals then in operation on the North American Continent, visited the United States and Canada, where they found in service Daboll's trumpets, steam whistles, and siren apparatus, devised by Mr. Felix Brown, of Progress Works, New York, sounded by steam and compressed air. From the report of the Trinity House Committee, it does not appear that they were greatly impressed with this instrument, but probably they had not an opportunity of testing its real merits, as compared with other signals. The late Professor Henry, of the United States Lighthouse Board, entertained a very high opinion of the siren, and on his advice, and the urgent recommendation of Professor Tyndall, one of these instruments was sent to England and included in the fog signal experiments at the South Foreland in 1873–4. This investigation was carried out by the Trinity House, with the view of obtaining definite knowledge as to the relative merits of various sound-producing instruments then in use, and also of ascertaining how the propagation of sound is affected by meteorological phenomena. Professor Tyndall, as scientific adviser of the Trinity House, conducted the investigation, aided by a committee of the Trinity House and their engineer. These experiments were extended over a lengthened period, in all conditions of weather, and the well-

known scientific and practical results obtained, together with the ascertained relative merits of sound-producing instruments for the service of the mariner, have proved to be of the highest scientific interest and practical importance. The investigation at the South Foreland was followed up by the Trinity House with further explosive fog signal experiments, in which they were assisted by the authorities at Woolwich Arsenal, with guns of various forms, weight of charges, and descriptions of gunpowder. The powders tested were —(1) fine grain, (2) larger grain, (3) rifle large grain, and (4) pebble. The result placed the sound-producing powers of these powders exactly in the order above stated; the fine grain, or most rapidly burning powder, gave indisputably the loudest sound, while the report of the slowly burning pebble powder, was weakest of all. Here, again, the greater value of increased rapidity of combustion in producing sound, was demonstrated. It was found that charges of gun-cotton yielded reports louder at all ranges than equal charges of the best gunpowder; and further experiments proved the explosion of half-a-pound of gun-cotton gave a sound equal in intensity to that produced by three pounds of the best gunpowder. These investigations led the Trinity House to adopt gun-cotton for fog signals at isolated stations on rocks and shoals, as already described, where, from want of space, it had hitherto been possible to apply nothing better than a bell, or gong. Of all the sound signals now employed, for the warning and guidance of mariners during fog, viz. bells, gongs, guns, whistles, reed trumpets, sirens, and sounds produced by the explosion of gun-cotton, the blasts of the siren, and explosions of gun-cotton, have been found to be the most efficient for coast fog signals; therefore these signals have received the greatest care and attention in their development. The siren doubtless ranks first, for stations wherever it can be applied, chiefly on account of its economy in maintenance, and the facility it affords for giving prolonged blasts of any desired intensity or pitch, and thus providing any number of trustworthy distinctive characters that may be required to ensure individuality in the signal. Sirens are now employed at many floating and shore stations of the Trinity House, and one recently installed at Saint Catherine's Lighthouse, Isle of Wight, of the automatic Holmes type, of which we have here a model, absorbs during its blast not less than 600 horse-power. The audibility of the blasts of this instrument may be considered to be trustworthy at a range of two miles under all conditions of foggy atmosphere on the sea surface over which it is intended to be sounded. It is very desirable that for many landport stations a greater trustworthy range be provided for the mariner, but this can only be afforded by such increased power as would be required for a more powerful electric light installation to serve the mariner in other gradations of thick atmosphere. A very important improvement and economy have lately been effected in the sirens of the Trinity House by rendering them always instantaneously available for

sounding at their maximum power. This is accomplished by the storage of a sufficient quantity of compressed air to work the siren during the time required for raising steam and starting the engine. The signal is thus always in readiness for immediate action, day or night, with an expenditure of fuel only incurred during fog, which on the coast of this country does not exceed an average of 440 hours per annum. The experience yet gained with the most powerful fog signals now in use, although these apparatus far exceed in efficiency, for the service of the mariner in fog, any light that science can provide, is not yet so satisfactory as we could desire. The best signal is, as I have already stated, occasionally not heard, under certain atmospheric conditions, beyond two miles; while under other conditions, not apparent to the mariner, the signal is distinctly audible at ten miles: therefore there is much to be desired in the development of the means of propagating sound waves, and in rendering them audible to the mariner. In conclusion, I would venture to state that, with the best light and sound signals that can be provided, there are conditions of the atmosphere in which the mariner will earnestly look and listen in vain for the desired light or sound signal, and he must still, under such circumstances, exercise caution in availing himself of their guidance, and never neglect the assistance always at hand of his old trusty friend the lead.

[J. N. D.]

WEEKLY EVENING MEETING,

Friday, March 22, 1889.

Colonel J. A. Grant, C.B. C.S.I. F.R.S. Vice-President, in the Chair.

Eadweard Muybridge, Esq.

The Science of Animal Locomotion in its Relation to design in Art.

(Illustrated by the Zoopraxiscope.)

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, March 29, 1889.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

A. Gordon Salamon, Esq. F.C.S. *M.R.I.*

Yeast.

[Abstract deferred.]

GENERAL MONTHLY MEETING,

Monday, April 1, 1889.

SIR JAMES CRICHTON BROWNE, M.D. LL.D. F.R.S. Vice-President, in the Chair.

Septimus Felix Beevor, Esq. B.A.
Professor J. A. Fleming, M.A. D.Sc.
John Langston, Esq. J.P. F.R.C.S.
Major S. Flood Page,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to JOSEPH DAVIS, Esq. for his present of an American "Forecast" Barometer.

The following Arrangements for the Lectures after Easter were announced:—

JEAN PAUL RICHTER, Esq. Ph.D.—Three Lectures on THE ITALIAN RENAISSANCE PAINTERS: their associations, their education, and their employments (with Illustrations); on Tuesdays, April 30, May 7, 14.

PROFESSOR E. RAY LANKESTER, M.A. LL.D. F.R.S.—Four Lectures on SOME RECENT BIOLOGICAL DISCOVERIES; on Tuesdays, May 21, 28, June 4, 11.

EADWEARD MUYBRIDGE, Esq. of the University of Pennsylvania—Two Lectures on THE SCIENCE OF ANIMAL LOCOMOTION IN ITS RELATION TO DESIGN IN ART (illustrated by the Zoopraxiscope); on Thursdays, May 2, 9.

PROFESSOR DEWAR, M.A. F.R.S. *M.R.I.* Fullerian Professor of Chemistry, R.I. Jacksonian Professor of Natural Experimental Philosophy, Cambridge—Five Experimental Lectures on CHEMICAL AFFINITY; on Thursdays, May 16. 23, 30, June 6, 13.

JOSEPH BENNETT, Esq.—Four Lectures on THE ORIGIN AND DEVELOPMENT OF OPERA IN ENGLAND (with Musical Illustrations); on Saturdays, May 4, 11, 18, 25.

PROFESSOR W. KNIGHT, LL.D. Professor of Moral Philosophy at the University of St. Andrews—Three Lectures on I. THE CLASSIFICATION OF THE SCIENCES, HISTORICAL AND CRITICAL; II. IDEALISM AND EXPERIENCE, IN PHILOSOPHY AND LITERATURE; III. IDEALISM AND EXPERIENCE, IN ART AND LIFE (the Tyndall Lectures); on Saturdays, June 1, 8, 15.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz:—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XXI. Part 4. 8vo. 1889.
American Philosophical Society—Proceedings, Vol. XXV. No. 128. 8vo. 1888.
Arbuthnot, F. F. Esq. M.R.I.—The Behâristan (Abode of Spring). By Jâmi. 8vo. Benares, 1887.
The Gulistân (Rose Garden of Sadi). 8vo. Benares, 1888.
Asiatic Society, Royal (China Branch)—Journal, Vol. XXIII. No. 1. 8vo. 1888.
Astronomical Society, Royal—Monthly Notices, Vol. XLIX. No. 4. 8vo. 1889.
Bankers, Institute of—Journal, Vol. X. Part 3. 8vo. 1889.
Batavia Observatory—Rainfall in the East Indian Archipelago, 1887. 8vo. 1888.
Magnetical and Meteorological Observations, Vols. VIII. and X. fol. 1888.
British Architects, Royal Institute of—Proceedings, 1888–9, Nos. 10, 11. 4to.
Cambridge Philosophical Society—Transactions, Vol. XIV. Part 3. 4to. 1889.
Chemical Society—Journal for March, 1889. 8vo.
Church, Professor A. H. M.A. F.R.S. M.R.I. (the Author)—Guide to the Museum of Roman Remains at Cirencester. 7th ed. 8vo. 1889.
Civil Engineers' Institution—Minutes of Proceedings, Vol. XCV. 8vo. 1889.
Clinical Society—Transactions, Vol. XXI. 8vo. 1888.
Dax: Société de Borda—Bulletin, Quatorzieme Année, 1r Tremestre. 8vo. 1889.

Editors—American Journal of Science for March, 1889. 8vo.
Analyst for March, 1889. 8vo.
Athenæum for March, 1889. 4to.
Chemical News for March, 1889. 4to.
Chemist and Druggist for March, 1889. 8vo.
Electrical Engineer for March, 1889. fol.
Engineer for March, 1889. fol.
Engineering for March, 1889. fol.
Horological Journal for March, 1889. 8vo.
Industries for March, 1889. fol.
Iron for March, 1889. 4to.
Murray's Magazine for March, 1889. 8vo.
Nature for March, 1889. 4to.
Photographic News for March, 1889. 8vo.
Revue Scientifique for March, 1889. 4to.
Telegraphic Journal for March, 1889. 8vo.
Zoophilist for March, 1889. 4to.

Florence, Biblioteca Nazionale Centrale—Bolletino, Num. 77. 8vo. 1889.

Geographical Society, Royal—Proceedings, New Series, Vol. XI. No. 3. 8vo. 1889.

Geological Institute, Imperial, Vienna—Verhandlungen, 1889, No. 2. 8vo.

Hamilton, Colonel A. C. R.E. M.R.I. (the Author)—Map of the World on a new projection, showing Zoogeographical regions. (2 copies.) 1889.

Mensbrugghe, G. van der, Esq. (the Author)—L'Influence de la Capillarité dans la Densimetrie. 8vo. 1888.
Contribution à la Théorie du Siphon. 8vo. 1889.

Miller, W. J. C. Esq. (the Registrar)—Medical Register, 1889. 8vo.
Dentists' Register, 1889. 8vo.
Medical Students' Register, 1889. 8vo.
Fourth Report of General Medical Council. 8vo. 1888.

Odontological Society of Great Britain—Transactions, Vol. XXI. No. 5. New Series. 8vo. 1889.

Pharmaceutical Society of Great Britain—Journal, March, 1889. 8vo.

Royal Colonial Institute—Proceedings, Vols. I. II. IV. VI. to XIX. 8vo. 1869–88.

Royal Society of London—Proceedings, No. 276. 8vo. 1889.

Saxon Society of Sciences, Royal—Mathematisch-physische Classe: Abhandlung. Band XV. Nos. 1, 2. 8vo. 1888. Berichte, 1888, Nos. 1, 2. 8vo. 1889.
Philologisch-historischen Classe: Berichte, 1888, Nos. 3, 4. 8vo. 1889.

Siemens, Alexander, Esq. M.R.I. (for the Executors)—Scientific Works of Sir William Siemens. Edited by E. F. Bamber. 3 vols. 8vo. 1889.

Society of Architects—Proceedings, Vol. I. No. 8. 8vo. 1889.

Society of Arts—Journal, March, 1889. 8vo.

Society of Chemical Industry—Journal, Vol. VIII. Nos. 1, 2. 8vo. 1889.

Thompson, Sir Henry, F.R.C.S. M.R.I. (the Author)—Modern Cremation: its History and Practice. 8vo. 1889.

United States Geological Survey—Mineral Resources of the United States, 1885. 8vo. 1886.
Bulletins, Nos. 40–47. 8vo. 1887–8.

Wright and Co. Messrs. J. (the Publishers)—The Medical Annual for 1889. 8vo.
Lectures on Bright's Disease. By R. Saundby. 8vo. 1889.

WEEKLY EVENING MEETING,

Friday, April 5, 1889.

Colonel J. A. Grant, C.B. C.S.I. F.R.S. Vice-President, in the Chair.

The Rev. Canon Ainger, M.A.

True and False Humour in Literature.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, April 12, 1889.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

The Right Hon. Lord Rayleigh, M.A. D.C.L. LL.D. F.R.S. *M.R.I.*
PROFESSOR OF NATURAL PHILOSOPHY, R.I.

Iridescent Crystals.

The principal subject of the lecture is the peculiar coloured reflection observed in certain specimens of chlorate of potash. Reflection implies a high degree of discontinuity. In some cases, as in decomposed glass, and probably in opals, the discontinuity is due to the interposition of layers of air; but, as was proved by Stokes, in the case of chlorate crystals the discontinuity is that known as twinning. The seat of the colour is a very thin layer in the interior of the crystal and parallel to its faces.

The following laws were discovered by Stokes:—

(1) If one of the crystalline plates be turned round in its own plane, without alteration of the angle of incidence, the peculiar reflection vanishes twice in a revolution, viz. when the plane of incidence coincides with the plane of symmetry of the crystal. [Shown.]

(2) As the angle of incidence is increased the reflected light becomes brighter and rises in refrangibility. [Shown.]

(3) The colours are not due to absorption, the transmitted light being strictly complementary to the reflected.

(4) The coloured light is not polarised. It is produced indifferently, whether the incident light be common light or light polarised in any plane, and is seen whether the reflected light be viewed directly or through a Nicol's prism turned in any way. [Shown.]

(5) The spectrum of the reflected light is frequently found to consist almost entirely of a comparatively narrow band. When the angle of incidence is increased, the band moves in the direction of increasing refrangibility, and at the same time increases rapidly in width. In many cases the reflection appears to be almost total.

In order to project these phenomena a crystal is prepared by cementing a smooth face to a strip of glass, whose sides are not quite parallel. The white reflection from the anterior face of the glass can then be separated from the real subject of the experiment.

A very remarkable feature in the reflected light remains to be noticed. If the angle of incidence be small, and if the incident light

be polarised in or perpendicularly to the plane of incidence, the reflected light is polarised in the *opposite* manner. [Shown.]

Similar phenomena, except that the reflection is white, are exhibited by crystals prepared in a manner described by Madan. If the crystal be treated beyond a certain point the peculiar reflection disappears, but returns upon cooling. [Shown.]

In all these cases there can be little doubt that the reflection takes place at twin surfaces, the theory of such reflection* reproducing with remarkable exactness most of the features above described. In order to explain the vigour and purity of the colour reflected in certain crystals, it is necessary to suppose that there are a considerable number of twin surfaces disposed at approximate equal intervals. At each angle of incidence there would be a particular wave length for which the phases of the several reflections are in agreement. The selection of light of a particular wave length would thus take place upon the same principle as in diffraction spectra, and might reach a high degree of perfection.

In illustration of this explanation an acoustical analogue is exhibited. The successive twin planes are imitated by parallel and equidistant discs of muslin (Figs. 1 and 2) stretched upon brass rings

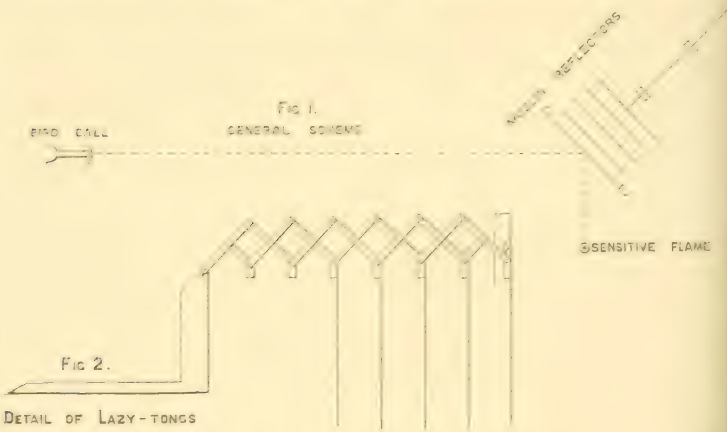

Fig. 1. General Scheme.

Fig. 2. Detail of Lazy-tongs

and mounted (with the aid of three lazy-tongs arrangements), so that there is but one degree of freedom to move, and that of such a character as to vary the interval between the discs without disturbing their equidistance and parallelism.

The source of sound is a bird-call, giving a pure tone of high pitch (inaudible), and the percipient is a high pressure flame issuing from a burner so oriented that the direct waves are without influence

* 'Phil. Mag.' Sept. 1888.

upon the flame.* But the waves reflected from the muslin arrive in the effective direction, and if of sufficient intensity induce flaring. The experiment consists in showing that the action depends upon the distance between the discs. If the distance be such that the waves reflected from the several discs co-operate,† the flame flares, but for intermediate adjustments recovers its equilibrium. For full success it is necessary that the reflective power of a single disc be neither too great nor too small. A somewhat open fabric appears suitable.

It was shown by Brewster that certain natural specimens of Iceland spar are traversed by thin twin strata. A convergent beam, reflected at a nearly grazing incidence from the twin planes, depicts upon the screen an arc of light, which is interrupted by a dark spot corresponding to the plane of symmetry. [Shown.] A similar experiment may be made with small rhombs in which twin layers have been developed by mechanical force after the manner of Reusch.

The light reflected from fiery opals has been shown by Crookes to possess in many cases a high degree of purity, rivalling in this respect the reflection from chlorate of potash. The explanation is to be sought in a periodic stratified structure. But the other features differ widely in the two cases. There is here no semicircular evanescence, as the specimen is rotated in azimuth. On the contrary, the coloured light transmitted perpendicularly through a thin plate of opal undergoes no change when the gem is turned round in its own plane. This appears to prove that the alternate states are not related to one another as twin crystals. More probably the alternate strata are of air, as in decomposed glass. The brilliancy of opals is said to be readily affected by atmospheric conditions.

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* See 'Proc. Roy. Inst.' Jan. 1888.

† If the reflection were perpendicular, the interval between successive discs would be equal to the half wave-length, or to some multiple of this.

ANNUAL MEETING,

Wednesday, May 1, 1889.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1888, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 81,000*l.* entirely derived from the Contributions and Donations of the Members.

Forty-five new Members were elected in 1888.

Sixty-four Lectures and Nineteen Evening Discourses were delivered in 1888.

The Books and Pamphlets presented in 1888 amounted to about 296 volumes, making, with 570 volumes (including Periodicals bound) purchased by the Managers, a total of 866 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, K.G. D.C.L. LL.D.
Treasurer—Henry Pollock, Esq.
Secretary—Sir Frederick Bramwell, Bart. D.C.L. F.R.S. M. Inst. C.E.

Managers.

Sir Frederick Abel, C.B. D.C.L. F.R.S.
Sir James Crichton Browne, M.D. LL.D. F.R.S.
William Crookes, Esq. F.R.S.
Francis Galton, Esq. M.A. F.R.S.
Colonel James A. Grant, C.B. C.S.I. F.R.S.
The Rt. Hon. Sir Wm. R. Grove, M.A. D.C.L. F.R.S.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
David Edward Hughes, Esq. F.R.S.
Rev. John Macnaught, M.A.
William Henry Preece, Esq. F.R.S. M. Inst. C.E.
William O. Priestley, M.D. LL.D. F.L.S.
John Rae, M.D. LL.D. F.R.S.
William Chandler Roberts-Austen, Esq. F.R.S.
Lord Arthur Russell.
Basil Woodd Smith, Esq. F.R.A.S.

Visitors.

William Anderson, Esq. M. Inst. C.E.
John Birkett, Esq. F.R.C.S.
Alfred Carpmael, Esq.
Ernest H. Goold, Esq. F.Z.S.
Charles Hawksley, Esq. M. Inst. C.E.
John Hopkinson, Esq. M.A. F.R.S. M.Inst. C.E.
Victor Horsley, Esq. F.R.S. F.R.C.S.
Ludwig Mond, Esq. F.C.S.
Edward Pollock, Esq.
Lachlan Mackintosh Rate, Esq. M.A.
Arthur William Rücker, Esq. M.A. F.R.S.
John Bell Sedgwick, Esq. J.P. F.R.G.S.
Thomas Edward Thorpe, Esq. Ph.D. F.R.S.
Thomas Tyrer, Esq. F.C.S.
James Wimshurst, Esq.

WEEKLY EVENING MEETING,

Friday, May 3, 1889.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Sir Henry Roscoe, M.P. D.C.L. LL.D. V.P.R.S.

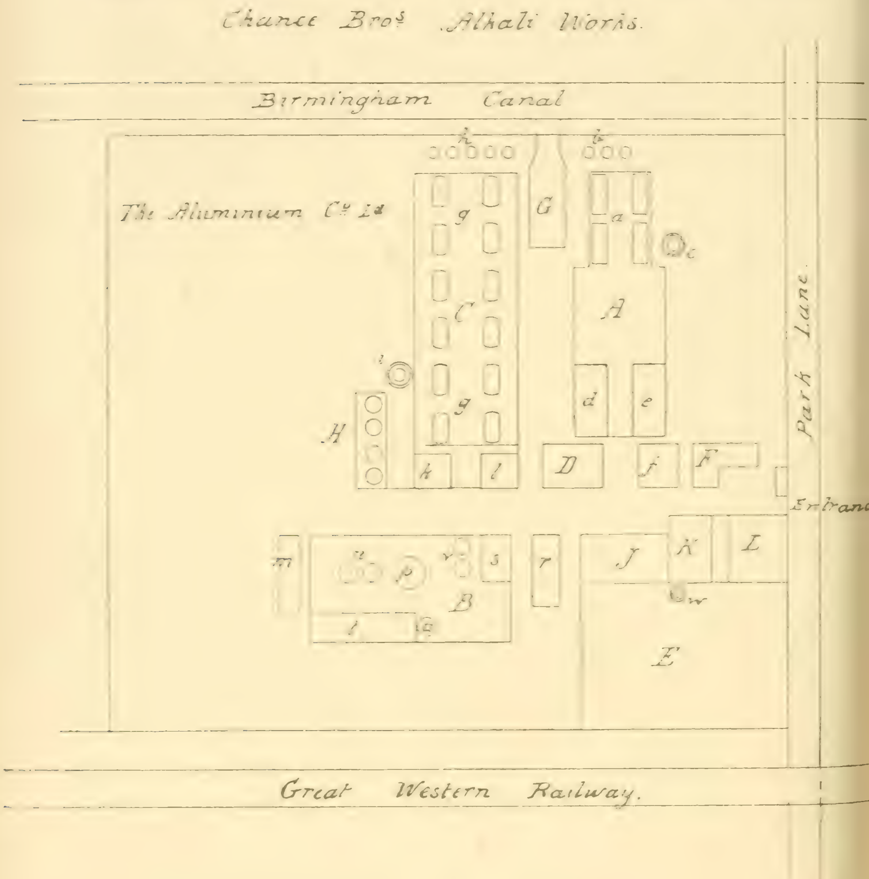
Aluminium.

Chemists of many lands have contributed to our knowledge of the metal aluminium. Davy, in 1807, tried in vain to reduce alumina by means of the electric current. Oerstedt, the Dane, in 1824, pointed out that the metal could be obtained by treating the chloride with an alkali metal; this was accomplished in Germany by Wöhler in 1827, and more completely in 1845, whilst in 1854, Bunsen showed how the metal can be obtained by electrolysis. But it is to France, by the hands of Henri St. Claire Deville, in the same year, that the honour belongs of having first prepared aluminium in a state of purity, and of obtaining it on a scale which enabled its valuable properties to be recognised and made available, and the bar of "silver-white metal from clay," was one of the chemical wonders in the first Paris Exhibition of 1855. Now England and America step in, and I have this evening to relate the important changes which further investigation has effected in the metallurgy of aluminium. The process suggested by Oerstedt, carried out by Wöhler, and modified by Deville, remains in principle unchanged. The metal is prepared, as before, by a reduction of the double chloride of aluminium and sodium, by means of metallic sodium in presence of cryolite; and it is therefore not so much a description of a new reaction as of improvements of old ones of which I have to speak.

I may perhaps be allowed to remind my hearers that more than 33 years ago, Mr. Barlow, then secretary to the Institution, delivered a discourse, in the presence of M. Deville, on the properties and mode of preparation of aluminium, then a novelty. He stated that the metal was then sold at the rate of 3*l.* per ounce, and the exhibition of a small ingot, cast in the laboratory by M. Deville, was considered remarkable. As indicating the progress since made, I may remark that the metal is now sold at 20*s.* per lb., and manufactured by the ton, by the Aluminium Company, at their works at Oldbury, near Birmingham. The improvements which have been made in this manufacture by the zeal and energy of Mr. Castner, an American metallurgist, are of so important a character, that the process may properly be termed the Deville-Castner process.

The production of aluminium previous to 1887, probably did not exceed 10,000 lbs. per annum, whilst the price at that time was very

high. To attain even this production required that at least 100,000 lbs. of double chloride, and 40,000 lbs. of sodium should be manufactured annually. From these figures an idea of the magnitude of the undertaking assumed by the Aluminium Company may be estimated, when we learn that they erected works having an annual



producing capacity of 100,000 lbs. of aluminium. To accomplish this, required not only that at least 400,000 lbs. of sodium, 800,000 lbs. of chlorine, and 1,000,000 lbs. of double chloride, should be annually manufactured, but in addition that each of these materials should be produced at a very low cost, in order to enable the metal to be sold at 20*s.* per lb.

Annexed is a sketch plan of the works, which now cover a space of nearly five acres. They are divided into five separate departments, viz., 1st, sodium, A; 2nd, chlorine, B; 3rd, chloride, C; 4th, aluminium, D; and 5th, foundry, rolling, wire mills, &c., E.

In each department an accurate account is kept of the production each day, the amount of material used, the different furnaces and apparatus in operation, &c. In this manner it has been found possible to ascertain each day exactly how the different processes are progressing, and what effect any modification has, either on cost, quantity, or quality of product. By this means a complicated chemical process is reduced to a series of very simple operations, so that whilst the processes are apparently complicated and difficult to carry out successfully, this is not the case now that the details connected with the manufacture have been perfected, and each operation carried on quite independently until the final materials are brought together for the production of the aluminium.

Manufacture of Sodium.

The first improvement occurs in the manufacture of sodium by what is known as the "Castner Process." The successful working of this process marks an era in the production of sodium, as it not only has greatly cheapened the metal, but has enabled the manufacture to be carried out upon a very large scale with little or no danger. Practically, the process consists in heating fused caustic soda in contact with carbon whilst the former substance is in a perfectly liquid condition. By the process in vogue before the introduction of this method, it was always deemed necessary that special means should be taken to guard against actual fusion of the mixed charges, which, if it were to take place, would to a large extent allow the alkali and reducing material to separate. Thus having an infusible charge to heat, requiring the employment of a very high temperature for its decomposition, the iron vessels must be of small circumference to allow the penetration of the heat to the centre of the charge without actually melting the vessel in which the materials are heated. By the new process, owing to the alkali being in a fused or perfectly liquid condition in contact directly with carbon, the necessity of this is avoided, and consequently, the reduction can be carried on in large vessels at a comparatively low temperature. The reaction taking place may be expressed as follows:—

$$3NaHO + C = Na_2CO_3 + 3H + Na.$$

The vessels in which the charges of alkali and reducing material are heated are of egg-shaped pattern, about 18 inches in width at their widest part and about 3 feet high, and are made in two portions, the lower one being actually in the form of a crucible, while the upper one is provided with an upright stem and a protruding hollow arm. This part of the apparatus is known as the cover. In com-

mencing the operation, these covers are raised in the heated furnace through apertures provided in the floor of the heated chamber, and are then fastened in their place by an attachment adjusted to the stem; the hollow arm extends outside the furnace. Directly below each aperture in the bottom of the furnace are situated the hydraulic lifts; attached to the top of which are the platforms upon which are placed the crucibles to be raised into the furnace. Attached to the hydraulic lifts are the usual reversing valves for lowering or raising, and the platform is of such a size as, when raised, completely to fill the bottom aperture of the furnace. The charged crucible, being placed upon the platform, is raised into its position, the edges meeting those of the cover, forming an air-tight joint which prevents the escape of gas and vapour from the vessel during reduction, except by the hollow arm provided for this purpose. The natural expansion of the iron vessels is accommodated by the water-pressure in the hydraulic lifts, so that the joint of the cover and crucible are not disturbed until it is intended to lower the lift for the purpose of removing the crucible.

The length of time required for the first operation of reduction and distillation is about two hours. At the end of this time the crucibles are lowered, taken from the platforms by a large pair of tongs on wheels, carried to a dumping pit, and thrown on their side. The residue is cleaned out, and the hot pot, being again gripped by the tongs, is taken back to the furnace. On its way, the charge of alkali and reducing material is thrown in. It is again placed on the lift and raised in position against the edges of the cover. The time consumed in making the change is $1\frac{1}{2}$ minute, and it only requires about seven minutes to draw, empty, recharge, and replace the five crucibles in each furnace. In this manner the crucibles retain the greater amount of their heat, so that the operation of reduction and distillation now only requires one hour and ten minutes. Each of the four furnaces, of five crucibles each, when in operation, are drawn alternately, so that the process is carried on night and day.

Attached to the protruding hollow arm from the cover are the condensers, which are of a peculiar pattern specially adapted to this process, being quite different from those formerly used. They are about 5 inches in diameter, and nearly 3 feet long, and have a small opening in the bottom about 20 inches from the nozzle. The bottom of these condensers is so inclined that the metal condensed from the vapour issuing from the crucible during reduction, flows down and out into a small pot placed directly below this opening. The uncondensed gases escape from the condenser at the further end, and burn with the characteristic sodium flame. The condensers are also provided with a small hinged door at the further end, by means of which the workmen from time to time may look in to observe how the distillation is progressing. Previous to drawing the crucibles from the furnace for the purpose of emptying and recharging, the small pots each containing the distilled metal are removed, and empty

ones substituted. Those removed each contain on an average about 6 lbs. of metal, and are taken directly to the sodium casting shop, where it is melted and cast, either into large bars ready to be used for making aluminium, or in smaller sticks to be sold.

Special care is taken to keep the temperature of the furnaces at about 1000° C., and the gas and air valves are carefully regulated, so as to maintain as even a temperature as possible. The covers remain in the furnace from Sunday night to Saturday afternoon, and the crucibles are kept in use until they are worn out, when new ones are substituted without interrupting the general running of the furnace. A furnace in operation requires 250 lbs. of caustic soda every one hour and ten minutes, and yields in the same time 30 lbs. of sodium, and about 240 lbs. of crude carbonate of soda. With the four furnaces at work 120 lbs. of sodium can be made every 70 minutes, or over a ton in the 24 hours. The residual carbonate, on treatment with lime in the usual manner, yields two-thirds of the original amount of caustic operated upon. The sodium, after being cast, is saturated with kerosene oil, and stored in large tanks holding several tons, placed in rooms specially designed both for security against either fire or water.

Chlorine Manufacture.

This part of the works is connected with the adjacent works of Messrs. Chance Bros. by a large gutta-percha pipe, by means of which from time to time hydrochloric acid is supplied direct into the large storage cisterns, from which it is used as desired for making the chlorine. For the preparation of the chlorine gas needed in making the chloride, the usual method is employed; that is, hydrochloric acid and manganese dioxide are heated together, when chlorine gas is evolved with effervescence, and is led away by earthenware and lead pipes to large lead-lined gasometers, where it is stored.

The materials for the generation of the chlorine are brought together in large tanks, or stills, built up out of great sandstone slabs, having rubber joints, and the heating is effected by the injection of steam. The evolution of gas, at first rapid, becomes gradually slower, and at last stops; the hydrochloric acid and manganese dioxide being converted into chlorine and manganous chloride. This last compound remains dissolved in the "spent still liquor" and is reconverted into manganese dioxide, to be used over again, by Weldon's Manganese Recovery Process. Owing to the difficulty of keeping up a regular supply of chlorine under a constant pressure directly from the stills, in order that the quantity passed into the sixty different retorts in which the double-chloride is made can be regulated and fed as desired, four large gasometers were erected. Each of these is capable of holding 1,000 cubic feet of gas, and is completely lined with lead, as are all the connecting mains, &c., this being the only available metal which withstands the corrosive action of chlorine. The

gasometers are filled in turn from the stills, the chlorine consumed being taken direct from a gasometer under a regular pressure until it is exhausted; the valves being changed, the supply is taken from another holder, the emptied one being refilled from the still.

Manufacture of the Double Chloride.

Twelve large regenerative gas furnaces are used for heating, and in each of these are fixed five horizontal fire-clay retorts about 10 ft. in length, into which the mixture for making the double chloride is placed. These furnaces have been built in two rows, six on a side, the clear passage-way down the centre of the building, which is about 250 ft. long, being 50 ft. in width. Above this central passage is the staging carrying the large lead-mains for the supply of the chlorine coming from the gasometers. Opposite each retort, and attached to the main, are situated the regulating valves, connected with lead and earthenware pipes, for the regulation and passage of the chlorine to each retort. The valves are of peculiar design, and have been so constructed that the chlorine is made to pass through a certain depth of liquid, which not only by opposing a certain pressure allows a known quantity of gas to pass in a given time, but also prevents any return from the retort into the main, should an increase of pressure be suddenly developed in the retorts.

The mixture with which the retorts are charged is made by grinding together hydrate of alumina, salt, and charcoal. This mixture is then moistened with water, which partially dissolves the salt, and thrown into a pug mill of the usual type for making drain pipes, excepting that the mass is forced out into solid cylindrical lengths upon a platform alongside of which a workman is stationed with a large knife, by means of which the material is cut into lengths of about 3 inches each. These are then piled on top of the large furnaces to dry. In a few hours they have sufficiently hardened to allow of their being handled. They are then transferred to large wagons, and are ready to be used in charging the retorts.

The success of this process is in a great measure dependent—1st, on the proportionate mixture of materials; 2nd, on the temperature of the furnace; 3rd, on the quantity of chlorine introduced in a given time; and 4th, on the actual construction of the retorts. I am, however, not at liberty to discuss the details of this part of the process, which have only a commercial interest. In carrying on the operation, the furnaces or retorts, when at the proper temperature, are charged by throwing in the balls until they are quite full, the fronts are then sealed up, and the charge allowed to remain undisturbed for about four hours, during which time the water of the alumina hydrate is completely expelled. At the end of this time the valves on the chlorine main are opened, and the gas is allowed to pass into the charged retorts. In the rear of each retort, and connected therewith by means of an earthenware pipe, are the condenser

boxes, which are built in brick. These boxes are provided with openings or doors, and also with earthenware pipes connected with a small flue for carrying off the uncondensed vapours to the large chimney. At first the chlorine passed into each retort is all absorbed by the charge, and only carbonic oxide escapes into the open boxes, where it burns. After a certain time, however, dense fumes are evolved, and the boxes are then closed, while the connecting pipe between the box and the small flue serves to carry off the uncondensed vapours to the chimney.

The reaction which takes place is as follows :—

$$Al_2O_3 + 2NaCl + 3C + 6Cl = 2AlCl_3NaCl + 3CO.$$

The chlorine is passed in for about 72 hours in varying quantity, the boxes at the back being opened from time to time by the workmen to ascertain the progress of the distillation. At the end of the time mentioned the chlorine valves are closed and the boxes at the back of the furnace are all thrown open. The crude double chloride, as distilled from the retorts, condenses in the connecting pipe and trickles down into the boxes, where it solidifies in large irregular masses. The yield from a bench of five retorts will average from 1,600 to 1,800 lbs., which is not far from the theoretical quantity. After the removal of the crude chloride from the condenser boxes, the retorts are opened at their charging end, and the residue, which consists of a small quantity of alumina, charcoal, and salt, is raked out and remixed in certain proportions with fresh material, to be used over again. The furnace is immediately re-charged and the same operations repeated, so that from each furnace upwards of 3,500 lbs. of chloride are obtained weekly. With ten of the twelve furnaces always at work the plant is easily capable of producing 30,000 lbs. of chloride per week, or 1,500,000 lbs. per annum.

Owing to the presence of iron, both in the materials used (viz., charcoal, alumina, &c.) and in the fireclay composing the retorts, the distilled chloride always contains a varying proportion of this metal in the form of ferrous and ferric chlorides. When it is remembered that it requires 10 lbs. of this chloride to produce 1 lb. of aluminium by reduction, it will be quite apparent how materially a very small percentage of iron in the chloride will influence the quality of the resulting metal. I may say that, exercising the utmost care as to the purity of the alumina and the charcoal used, and after having the retorts made of special fireclay containing only a very small percentage of iron, it was found almost impossible to produce upon a large scale a chloride containing less than 0·3 per cent. of iron.

This crude double chloride, as it is now called at the works, is highly deliquescent, and varies in colour from a light yellow to a dark red. The variation in colour is not so much due to the varying percentage of iron contained as to the relative proportion of ferric or ferrous chlorides present, and although a sample may be either very dark or quite light, it may still contain only a small percentage of

iron if it be present as ferric salt, or a very large percentage if it is in the ferrous condition. Even when exercising all possible precautions, the average analysis of the crude double chloride shows about 0·4 per cent. of iron. The metal subsequently made from this chloride therefore never contained much less than about 5 per cent. of iron, and, as this quantity greatly injures the capacity of aluminium for drawing into wire. rolling, &c., the metal thus obtained required to be refined. This was successfully accomplished by Mr. Castner and his able assistant Mr. Cullen, and for some time all the metal made was refined, the iron being lowered to about 2 per cent.

The process, however, was difficult to carry out, and required careful manipulation, but as it then seemed the only remedy for effectively removing the iron, it was adopted and carried on for some time quite successfully, until another invention of Mr. Castner rendered it totally unnecessary. This consisted in purifying the double chloride before reduction. I cannot now explain this process, but I am able to show some of the product. This purified chloride, or pure double chloride, is, as you see, quite white, and is far less deliquescent than the crude, so that it is quite reasonable to infer that this most undesirable property is greatly due to the former presence of iron chlorides. I have seen large quantities containing upwards of 1½ per cent. of iron, or 150 lbs. to 10,000 of the chloride, completely purified from iron in a few minutes, so that, whilst the substance before treatment was wholly unfit for the preparation of aluminium, owing to the presence of iron, the result was, like the sample exhibited, a mass containing only 1 lb. of iron in 10,000, or 0·01 per cent. The process is extremely simple, and adds little or no appreciable cost to the final product. After treatment, this pure chloride is melted in large iron pots and run into drums similar to those used for storing caustic soda. As far as I am aware, it was generally believed to be an impossibility to remove the iron from anhydrous double chloride of aluminium and sodium, and few if any chemists have ever seen a pure white double chloride.

Aluminium Manufacture.

I now come to the final stage of the process, viz., the reduction of the pure double chloride by sodium. This is effected, not in a tube of Bohemian glass, as shown in Mr. Barlow's lecture in 1856, but in a large reverberatory furnace, having an inclined hearth about 6 feet square, the inclination being towards the front of the furnace, through which are several openings at different heights. The pure chloride is ground together with cryolite in about the proportions of two to one, and is then carried to a staging erected above the reducing furnace. The sodium, in large slabs or blocks, is run through a machine similar to an ordinary tobacco-cutting machine, where it is cut into small thin slices; it is then also transferred to the staging above the reducing furnace.

Both materials are now thrown into a large revolving drum, when they become thoroughly mixed. The drum being opened and partially turned, the contents drop out into a car on a tramway directly below. The furnace having been raised to the desired temperature, the dampers of the furnace are all closed to prevent the access of air, the heating gas also being shut off. The car is then moved out on the roof of the furnace until it stands directly over the centre of the hearth. The furnace roof is provided with large hoppers, and through these openings the charge is introduced as quickly as possible. The reaction takes place almost immediately, and the whole charge quickly liquefies. At the end of a certain time the heating gas is again introduced and the charge kept at a moderate temperature for about two hours. At the end of this period the furnace is tapped by driving a bar through the lower opening, which has previously been stopped with a fire-clay plug, and the liquid metal run out in a silver stream into moulds placed below the opening. When the metal has all been drawn off, the slag is allowed to run out into small iron wagons and removed. The openings being again plugged up, the furnace is ready for another charge. From each charge, composed of about 1200 lbs. of pure chloride, 600 lbs. of cryolite, and 350 lbs. of sodium, about 115 to 120 lbs. of aluminium is obtained.

The purity of the metal entirely depends upon the purity of the chloride used, and without exercising more than ordinary care the metal tests usually indicate a purity of metal above 99 per cent. On the table is the metal run from a single charge, its weight is 116 lbs., and its composition, as shown by analysis, is 99·2 aluminium, 0·3 silicon, and 0·5 iron. This I believe to be the largest and the purest mass of metal ever made in one operation.

The result of eight or nine charges are laid on one side, and then melted down in the furnace to make a uniform quality, the liquid metal, after a good stirring, being drawn off into moulds. These large ingots, weighing about 60 lb. each, are sent to the casting shop, there to be melted and cast into the ordinary pigs, or other shapes, as may be required for the making of tubes, sheets, or wire, or else used directly for making alloys of either copper or iron.

The following table shows approximately the quantity of each material used in the production of one ton of aluminium:—

Metallic sodium	6,300 lbs.
Double chloride	22,400 „
Cryolite	8,000 „
Coal	8 tons.

To produce 6,300 lbs. of sodium is required:—

Caustic soda	44,000 lbs.
Carbide made from pitch, 12,000 lbs., and iron turnings, 1,000 lbs.	7,000 „
Crucible castings	2½ tons.
Coal	75 „

For the production of 22,400 lbs. double chloride is required:—

Common salt	8,000 lbs.
Alumina hydrate	11,000 „
Chlorine gas	15,000 „
Coal	180 tons.

For the production of 15,000 lbs. of chlorine gas is required:—

Hydrochloric acid	180,000 lbs.
Limestone dust	45,000 „
Lime	30,000 „
Loss of manganese	1,000 „

(These figures were rendered more evident by the aid of small blocks, each cut a given size so as to represent the relative weights of the different materials used to produce one unit of aluminium.)

It might seem, on looking over the above numbers, as if an extraordinary amount of waste occurred, and as if the production is far below that which ought to be obtained, but a study of the figures will show that this is not the case. I would wish to call attention to one item in particular, viz. fuel, it having been remarked that the consumption of coal must prevent cheap production. I think when it is remembered that coal, such as used at the works, cost only 4*s.* per ton, while the product is worth 2240*l.* per ton, the cost of coal is not an item of consequence in the cost of production. The total cost of the coal to produce one ton of metal being 50*l.*; the actual cost for fuel is less than sixpence for every pound of aluminium produced. The ratio of cost of fuel to value of product is indeed less than is the case in making either iron or steel. In concluding my remarks as to the method of manufacture and the process in general, I may add that I do not think it is too much to expect, in view of the rapid strides already made, that in the future, further improvements and modifications will enable aluminium to be produced and sold even at a lower price than appears at present possible.

Properties of Aluminium.

In its physical properties aluminium widely differs from all the other metals. Its colour is a beautiful white, with a slight blue tint. The intensity of this colour becomes more apparent when the metal has been worked, or when it contains silicon or iron. The surface may be made to take a very high polish, when the blue tint of the metal become manifest, or it may be treated with caustic soda and then nitric acid, which will leave the metal quite white. The extensibility or malleability of aluminium is very high, ranking with gold and silver if the metal be of good quality. It may be beaten out into thin leaf quite as easily as either gold or silver, although it requires more careful annealing.

It is extremely ductile and may be easily drawn, especial care only being required in the annealing.

The excessive sonorousness of aluminium is best shown by example (large suspended bar being struck). Faraday has remarked, after experiments conducted in his laboratory, that the sound produced by an ingot of aluminium is not simple, and one may distinguish the two sounds by turning the vibrating ingot.

After being cast it has about the hardness of pure silver, but may be sensibly hardened by hammering.

Its tensile strength varies between 12 and 14 tons to the inch (test sample which was shown having been broken at 13 tons or 27,000 lbs.), ordinary cast iron being about 8 tons. Comparing the strength of aluminium in relation to its weight, it is equal to steel of 38 tons tensile strength. The specific gravity of cast aluminium is 2·58, but after rolling or hammering this figure is increased to about 2·68.

The specific gravity of aluminium being 1, copper is 3·6, nickel 3·5, silver 4, lead 4·8, gold 7·7.

The fusibility of aluminium has been variously stated as being between that of zinc and silver, or between 600 and 1000° C.

As no reliable information has ever been made public on this subject, my friend, Professor Carnelley, undertook to determine it. I was aware, from information gained at the works at Oldbury, that a small increase in the percentage of contained iron materially raised its point of fusion, and it has been undoubtedly due to this cause that such wide limits are given for the melting point. Under these circumstances two samples were forwarded for testing, of which No. 1, containing ½ per cent. of iron, had a melting point of 700° C.; whereas No. 2, containing 5 per cent. of iron, does not melt at 700° and only softens somewhat above that temperature but undergoes incipient fusion at 730°.

According to Faraday, aluminium ranks very high among metallic conductors of heat and electricity, and he found that it conducted heat better than either silver or copper. The specific heat is also very high, which accounts for length of time required for an ingot of the metal to either melt or get cold after being cast.

Chemically, its properties are well worthy of study.

Air, either wet or dry, has absolutely no effect on aluminium at the ordinary temperature, but this property is only possessed by a very pure quality of metal, and the pure metal in mass undergoes only slight oxidation even at the melting point of platinum.

Thin leaf, however, when heated in a current of oxygen, burns with a brilliant, bluish white light. (Experiment shown). If the metal be pure, water has no effect on it whatever, even at a red-heat. Sulphur and its compounds also are without action on it, while, under the same circumstances, nearly all metals would be discoloured with great rapidity. (Experiment shown using silver and aluminium under the same conditions.)

Dilute sulphuric acid and nitric acid, both diluted and concentrated, have no effect on it, although it may be dissolved in either hydrochloric acid or caustic alkali. Heated in an atmosphere of

chlorine it burns with a vivid light, producing aluminium chloride. (Experiment shown). In connection with the subject it may be of interest to state the true melting point of the double chloride of aluminium and sodium, which has always been given at 170° to 180° C., but which Mr. Baker, the chemist to the works, finds lies between 125° and 130° C.

Uses of Aluminium.

Its uses, unalloyed, have heretofore been greatly restricted. This is, I believe, alone owing to its former high price, for no metal possessing the properties of aluminium could help coming into larger use if its cost were moderate. Much has been said as to the impossibility of soldering it being against its popular use, but I believe that this difficulty will now soon be overcome. The following are a few of the purposes to which it is at present put: telescope tubes, marine glasses, eye glasses and sextants, especially on account of its lightness. Fine wire for the making of lace, embroidery, &c. Leaf in the place of silver leaf, sabre sheaths, sword handles, &c., statuettes and works of art, jewellery and delicate physical apparatus, culinary utensils, harness fittings, metallic parts of solders' uniforms, dental purposes, surgical instruments, reflectors (it not being tarnished by the products of combustion), photographic apparatus, aeronautical and engineering purposes, and especially for the making of alloys.

Alloys of Aluminium.

The most important alloys of aluminium are those made with copper. These alloys were first prepared by Dr. Percy, in England, and now give promise of being largely used. The alloy produced by the addition of 10 per cent. of aluminium to copper, the maximum amount that can be used to produce a satisfactory alloy, is known as aluminium bronze. Bronzes, however, are made which contain smaller amounts of aluminium, possessing in a degree the valuable properties of the 10 per cent. bronze. According to the percentage of aluminium up to 10 per cent., the colour varies from red gold to pale yellow. The 10 per cent. alloy takes a fine polish, and has the colour of jewellers' gold. The 5 per cent. alloy is not quite so hard, the colour being very similar to that of pure gold. I am indebted to Prof. Roberts Austen for a splendid specimen of crystallised gold, as also for a mould in which the gold at the mint is usually cast, and in this I have had prepared ingots of the 10 and 5 per cent. alloy, so that a comparison may be made of the colour of these with a gold ingot cast in the same mould, for the loan of which I have to thank Messrs. Johnson, Matthey, & Co., all of which are before you.

I have also ingots of the same size, of pure aluminium, from which an idea of the relative weights of gold and aluminium may be obtained.

To arrive at perfection in the making of these alloys, not only is

it required that the aluminium used should be of good quality, but also that the copper must be of the very best obtainable. For this purpose only the best brands of Lake Superior copper should be used. Inferior brands of copper or any impurities in the alloy give poor results. The alloys all possess a good colour, polish well, keep their colour far better than all other copper alloys, are extremely malleable and ductile, can be worked either hot or cold, easily engraved, the higher grades have an elasticity exceeding steel, are easily cast into complicated objects, do not lose in remelting, and are possessed of great strength, dependent, of course, on the purity and percentage of contained aluminium. The 10 per cent. alloy, when cast, has a tensile strength of between 70,000 and 80,000 lbs. per square inch, but when hammered or worked, the test exceeds 100,000 lbs. (A sample shown broke at 105,000 lbs.).

An attempt to enumerate either the present uses or the possible future commercial value of these alloys is beyond my present purpose. I may, however, remark that they are not only adapted to take the place of bronze, brass, and steel, but they so far surpass all of those metals, both physically and chemically, as to make their extended use assured. (Sheets, rods, tubes, wire, and ingots shown.)

But even a more important use of aluminium seems to be its employment in the iron industry, of which it promises shortly to become a valuable factor, owing to certain effects which it produces when present, even in the most minute proportions. Experiments are now being carried on at numerous iron and steel works, in England, on the Continent, and in America. The results so far attained are greatly at variance, for whilst in the majority of cases the improvements made have encouraged the continuance of the trials, in others the result has not been satisfactory. On this point I would wish to say to those who may contemplate making use of aluminium in this direction, that it would be advisable before trying their experiments to ascertain whether the aluminium alloy they may purchase actually contains any aluminium at all, for some of the so-called aluminium alloys contain little or no aluminium, and this may doubtless account for the negative results obtained. Again, others contain such varying proportions of carbon, silicon, and other impurities, as to render their use highly objectionable.

It seems to be a prevailing idea with some people, that because aluminium is so light compared with iron, that they cannot be directly alloyed, and furthermore, that for the same reason, alloys made by the direct melting together of the two metals would not be equal to an alloy where both metals are reduced together. Now, of course, this is not the case, and the statement has been put forward by those who were only able to make the alloys in one way.

Aluminium added to molten iron and steel lowers their melting points, consequently increases the fluidity of the metal, and causes it to run easily into moulds and set there, without entrapping air and other gases, which serve to form blow-holes and similar imper-

fections. It is already used by a large number of steel founders, and seems to render the production of sound steel castings more certain and easy than is otherwise possible.

One of the most remarkable applications of this property which aluminium possesses of lowering the melting-point of iron has been made use of by Mr. Nordenfelt in the production of castings of wrought iron.

Aluminium forms alloys with most other metals, and although each possess peculiar properties which in the future may be utilised, at present they are but little used.

In conclusion, I beg to call your attention to the wood models on the table, one being representative of aluminium, the other aluminium bronze. The originals of these models are now in the Paris Exhibition, each weighing 1000 lbs. With regard to aluminium bronze, I cannot speak positively, but the block of pure aluminium is undoubtedly the largest casting ever made in this most wonderful metal. I have to thank the Directors of the Aluminium Company, and especially Mr. Castner, for furnishing me with the interesting series of specimens of raw and manufactured metal for illustrating my discourse.

[H. E. R.]

GENERAL MONTHLY MEETING,

Monday, May 6, 1889.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Sir Frederick Abel, C.B. D.C.L. F.R.S.
Sir James Crichton Browne, M.D. LL.D. F.R.S.
William Crookes, Esq. F.R.S.
Colonel James A. Grant, C.B. C.S.I. F.R.S.
William Huggins, Esq. D.C.L. LL.D. F.R.S.
John Rae, M.D. LL.D. F.R.S.
Henry Pollock, Esq. Treasurer.
Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Hon. Secretary.

W. L. A. Bartlett Burdett-Coutts, Esq. M.P.
James J. Fellows, Esq.
Henry Parry Gilbey, Esq.
Colonel George Edward Gouraud,
Henry Arthur Hunt, Esq.
Thomas Stuart Kennedy, Esq. J.P.
Arthur Lucas, Esq.
Major-General Eardley Maitland, C.B. R.A.
Frederick Schwann, Esq.
Colonel William Brooke Thomson,
Charles Wilson Vincent, Esq. F.R.S.E. F.C.S. F.I.C.
R. W. Wallace, Esq.
Ernest Watney, Esq.

were elected Members of the Royal Institution.

The decease of Dr. Warren de la Rue, D.C.L. F.R.S. Manager and Vice-President, on April 19th, was announced from the Chair.

The following Resolution passed by the Managers at their Meeting this day was read :—

Resolved, "That the Managers of the Royal Institution of Great Britain have with much sorrow to record their profound sense of the loss sustained by the Institution, by themselves, and by the whole scientific world, in the decease of their friend and colleague, Dr. Warren de la Rue, who had been a valuable Member of the Institution for nearly thirty-eight years.

"Dr. Warren de la Rue's high position in regard to science is universally recognised. The fruits of his invaluable researches in the various departments of Practical Astronomy, Solar and Lunar Physics, Celestial Photography, Electricity, Chemistry, and Meteorology, are fully recorded in the 'Transactions' of the Royal Society, the Royal Astronomical Society, the Chemical Society, the Academy of Sciences, Paris, and in many other Scientific Journals.

"Dr. Warren de la Rue became a Member of the Royal Institution in 1851. Previous to his election he was much interested in the objects of the Institution, and zealously assisted Professor Faraday in his researches and lectures whenever an opportunity presented itself. He was first elected a Manager in 1856, and closely attended the meetings of his colleagues until the end of his life. In 1879 he became Honorary Secretary, and held that office till 1882. During his long connection with the Royal Institution he was specially interested in the affairs of the Laboratory, and was a liberal contributor to the Fund for the Promotion of Experimental Research, first started in 1863. He also frequently presented valuable apparatus, and eagerly embraced every opportunity of munificently contributing towards the supply of whatever was needed by the Professors. His discourse on 'The Phenomena of the Electric Discharge, with 14,000 Chloride of Silver Cells,' delivered at the Royal Institution on January 21, 1881, will be long remembered by those who had the good fortune to hear it and witness his brilliant experiments. Besides the great pleasure he derived from the prosecution of his own researches, for which he received many well-merited Honours both at home and abroad, Dr. de la Rue was happy in liberally aiding the labours of his fellow-workers, and contributed invaluable services to the Royal Institution while discharging the duties of Honorary Secretary and Manager and Vice-President. His genial manner and unvaried courtesy endeared him to all with whom he was personally connected, and his kindly presence will long be missed.

"The Managers further desire to be permitted to offer to Mrs. DE LA RUE and her family the expression of their most sincere sympathy with them in their bereavement."

Resolved, "That the Honorary Secretary be requested to communicate this Resolution to the family."

The Honorary Secretary was requested to convey the grateful thanks of the Members to Mrs. de la Rue for the generous spirit which prompted her to present the philosophical apparatus of the late Dr. Warren de la Rue, F.R.S. to the Royal Institution, and to inform her that this valuable gift will be carefully preserved as the historical collection commemorative of the important scientific work of Dr. Warren de la Rue, and of the eminent position which he has so long occupied as a promoter of Science, and of the special objects of the Royal Institution.

The Special Thanks of the Members were given to Mr. JOHN YOUNG for his valuable gift of a portrait of Sir Humphry Davy, presented by him in the name of his son, Mr. James Young, grandson of the late Dr. James Young, F.R.S. of Kelly, the distinguished chemist and former owner of the portrait, and the Honorary Secretary was requested to inform Mr. John Young that the Members have great satisfaction in receiving such a valuable addition to the collection of historic portraits in the Royal Institution, and that they are gratified in the presentation being associated with the name of the family of the late Dr. Young.

JOHN TYNDALL, Esq. D.C.L. LL.D. F.R.S. was elected Honorary Professor of Natural Philosophy.

The Right Hon. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. was elected Professor of Natural Philosophy.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz:—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XXII. Part 1. 4to. 1889.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1° Semestre, Vol. IV. Fasc. 11, 12; Vol. V. Fasc. 1, 2, 3. 8vo. 1889.

Astronomical Society, Royal—Monthly Notices, Vol. XLIX. No. 5. 8vo. 1889.

Bankers, Institute of—Journal, Vol. X. Part 4. 8vo. 1889.

Bavarian Academy of Sciences—Sitzungsberichte, 1888, Heft 3; 1889, Heft 1, 2. 8vo. 1889.

Bernays, Albert J. Esq. Ph.D. F.C.S. M.R.I. (the Author)—Notes on Analytical Chemistry.

Boston Society of Natural History—Proceedings, Vol. XXIII. Parts 3 and 4. 8vo. 1888.

British Museum (Natural History)—Catalogue of Fossil Fishes, Part 1. 8vo. 1889.

Catalogue of Fossil Cephalopoda, Part 1. 8vo. 1888.

Catalogue of Marsupialia and Monotremata. 8vo. 1888.

Catalogue of the Chelonians, Rhynchocephalians, and Crocodiles. 8vo. 1889.

Cambridge Philosophical Society—Proceedings, Vol. VI. Part 5. 8vo. 1889.
Chemical Industry, Society of—Journal, Vol. VIII. No. 3. 8vo. 1889.
Chemical Society—Journal for April, 1889. 8vo.
Cracovie, L'Academie des Sciences—Bulletin, 1889. 8vo.
Editors—American Journal of Science for April, 1889. 8vo.
Analyst for April, 1889. 8vo.
Athenæum for April, 1889. 4to.
Chemical News for April, 1889. 4to.
Chemist and Druggist for April, 1889. 8vo.
Electrical Engineer for April, 1889. fol.
Engineer for April, 1889. fol.
Engineering for April, 1889. fol.
Horological Journal for April, 1889. 8vo.
Industries for April, 1889. fol.
Iron for April, 1889. 4to.
Murray's Magazine for April, 1889. 8vo.
Nature for April, 1889. 4to.
Photographic News for April, 1889. 8vo.
Revue Scientifique for April, 1889. 4to.
Telegraphic Journal for April, 1889. 8vo.
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Franklin Institute—Journal, No. 760. 8vo. 1889.
Holmes-Forbes, A. W. Esq. M.A. M.R.I. (the Author)—Know Thyself; or Psychology for the People. 8vo. 1889.
Iowa Laboratories of Natural History—Bulletin, Vol. I. No 1. 8vo. 1888.
Latzina, M. F. (the Compiler)—Censo General de la Cuidad de Buenos Aires, 1887. 8vo. 1889.
Marvin, Charles, Esq. (the Author)—The Coming Oil Age. 8vo. 1889.
Meriden Scientific Association—Transactions, Vol. III. 8vo. 1887–8.
Meteorological Office—Hourly Readings, 1886, Part 2. 4to. 1889.
Quarterly Weather Reports, 1876, Part 4. 4to. 1889.
Meteorological Society, Royal—Quarterly Journal, No. 69. 8vo. 1889.
Meteorological Record, No. 31. 8vo. 1889.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quinta Vol. III. Nos. 1, 2. 8vo. And Disegni. fol. 1889.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXXVIII. Parts 1–2. 8vo. 1889.
Pharmaceutical Society of Great Britain—Journal for April, 1889. 8vo.
Physical Society of London—Proceedings, Vol. X. Part 1. 8vo. 1889.
Preussische Akademie der Wissenschaften—Sitzungsberichte, Nos. 38–52. 8vo. 1888.
Royal College of Physicians, Edinburgh—Reports from the Laboratory, Vol. I. 8vo. 1889.
Royal Society of London—Proceedings, No. 277. 8vo. 1889.
Saxon Society of Sciences, Royal—Mathematisch-physische Classe: Abhandlung. Band XV. Nos. 3, 4. 8vo. 1889.
Skinner, W. R. Esq. (the Editor)—The Mining Manual for 1888. 8vo.
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Statistical Society—Journal, Vol. LII. Part 1. 8vo. 1889.
Index to Vols. XXVI.–L. (1873–87). 8vo.
Telegraph Engineers, Society of—Journal, No. 78. 8vo. 1889.
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Zoological Society—Transactions, Vol. XII. Part 8. 4to. 1889.
Proceedings, 1888, Part 4. 8vo. 1889.

WEEKLY EVENING MEETING,

Friday, May 10, 1889.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

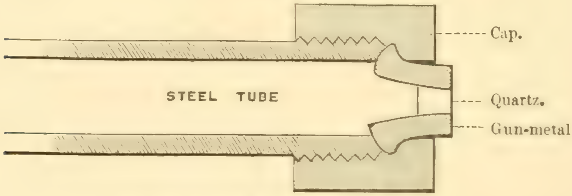
Professor Dewar, M.A. F.R.S. *M.R.I.*

Optical Properties of Oxygen and Ozone.

In the course of experiments on the spectra of gases at high pressures, Professor Liveing and I have made observations on the absorption-spectrum of oxygen which confirm and extend the observations of Egoroff and Jansen. The interest of this spectrum is so great, on account of the important part which oxygen plays in our world, and its free condition in our atmosphere, that it deserves a separate notice.

In order to include the ultra-violet rays in our observations we have had to contrive windows of quartz to the apparatus containing the gases. A strong steel tube, 165 centimetres long and 5 centimetres wide, was fitted with gun-metal ends, bearing by curved

Fig. 1.



Section through one end of the tube.

surfaces upon the conical openings of the tube, and forced home by powerful screw-caps. Each gun-metal end was pierced centrally by a conical opening fitted with a quartz stopper, 2·1 centimetres thick, and of the same diameter, with plane polished ends. A small amount of wax was interposed between the stopper and the gun-metal for the purpose of ensuring a uniform bearing for the quartz, which is very brittle. Trial proved that the tube thus fitted would sustain, without leakage, a pressure of upwards of 260 atmospheres. The tube had, besides, near each end, a screw-plug valve for admitting the gases. About the centre of the tube was placed a quartz lens, rather less in diameter than the tube, held in place by three springs which pressed against the walls of the tube. This lens had a focal length of about 46 centimetres; so that when a source of light was

placed about 10 centimetres from one end of the tube, an image of it was formed on the slit of the spectroscope at about the same distance from the other end of the tube, and thereby loss of light, so far as it was due to the distance of the source, was reduced to a minimum.

Ordinary oxygen was let into the tube from an iron bottle until the pressure reached 85 atmospheres, and on viewing an arc light through the tube the following absorptions were visible :—

(1) A very dark band sharply defined on its more refrangible side, gradually fading out on its less refrangible side, and divided into two parts by a streak of light, occupying the position of A of the solar spectrum.

(2) A much weaker, but precisely similar band in the position of B of the solar spectrum.

(3) A dark band very diffuse on both edges, extending from about λ 6360 to λ 6225, with a maximum intensity at about λ 6305.

(4) A still darker band a little above D, beginning with a diffuse edge at about λ 5810, rapidly coming to a maximum intensity at about λ 5785, and then gradually fading on the more refrangible side, and disappearing at about λ 5675.

(5) A faint narrow band in the green at about λ 5350.

(6) A strong band in the blue, diffuse on both sides, extending from about λ 4795 to λ 4750.

When photographs were taken of the ultra-violet part of the spectrum of the arc and of the iron spark, the gas appeared to be quite transparent for violet and ultra-violet rays up to about λ 2745. From that point the light gradually diminished, and beyond λ 2664 appeared to be wholly absorbed.

The pressure of the oxygen in the tube was then increased to 140 atmospheres. This had the effect of increasing sensibly the darkness of all the bands above described; but brought out no new bands, except a faint band in the indigo at about λ 4470. In the ultra-violet the absorption appeared to be complete for all rays beyond about λ 2704.

The foregoing observations were made with a spectroscope of small dispersion. We next brought to bear on the spectrum a large instrument with one of Rowland's gratings. Even with the high dispersion of this instrument, the bands at A could not be resolved into lines; they remained two diffuse bands; though the red potassium-lines, which were produced by sprinkling the electrode of the arc with a potassium-salt, were sharply defined and widely separated. None of the other bands were resolvable into lines. This we attribute to the density of the gas, by which the lines are expanded so as to obliterate the interspaces; and this supposition is confirmed by the observation of Ångström, that the band in the solar spectrum which appears to be identical with that observed by us a little above D, was resolved into fine lines when the sun was high, but appeared as a continuous band when the sun was near the horizon.

On letting down the pressure the bands were all weakened; A, though weaker, became more sharply defined at the more refrangible edge. The faint band in the indigo λ 4470 remained just visible until the pressure fell below 110 atmospheres. At 90 atmospheres A and B were still well seen and sharp, but all the other bands weaker. B remained visible until the pressure fell to 40 atmospheres. A was then still well seen, the band just above D very faint, and the others almost gone. At 30 atmospheres A was still easily seen, and there was a trace of the band above D. At 25 atmospheres this band had gone, but A remained visible until the pressure fell to less than 20 atmospheres. Hence an amount of oxygen not greater than that contained in a column of air 150 metres long at ordinary pressure, is sufficient to produce a visible absorption at A. The quantity of oxygen in the tube at the highest pressure we used falls, however, far short of the quantity traversed by the solar rays in passing through the atmosphere when the sun is vertical.

It will be noted that the bands, if we except the faint two in the green and indigo respectively, appear to be identical with those terrestrial bands in the solar spectrum which Ångström found to be as strong when the air was dried by intense frost as at other times. At least the positions of the maxima agree closely, and that near D shows the same peculiarity in having its maximum near the less refrangible end. We did not, however, observe a, which would be fainter than B, and if, like A and B, unresolvable, would be lost in the diffuse band which covers that region. The bands above numbered 3, 4, 5, 6, agree also with those observed by Olszewski,* to be produced by a layer of liquid oxygen 12 millimetres thick. The point also at which the absorption of the ultra-violet rays begins, agrees with that at which the absorption by ozone begins, as observed by Hartley †; but the oxygen, as we used it, did not appear to transmit the more refrangible rays beyond 2320, which seem to pass through ozone. Egoroff ‡ found that A remained visible when he looked through 80 metres of atmosphere, but 3 kilogrammes of atmosphere failed to produce a.

When the pressure in our tube was reduced, a cloud was always formed which rendered the contents of the tube nearly opaque; the faint light which was then transmitted had always a green tinge.

It is remarkable that the compounds of oxygen do not show any similar absorptions. Ångström thought it improbable that oxygen should have a spectrum of such a character, since he failed to obtain an emission spectrum resembling it; and suggested that the absorptions might be due to carbonic acid gas or to ozone, or possibly to oxygen in the state in which it becomes fluorescent.§ Neither carbonic acid gas nor nitrous oxide, at a pressure of 50 atmospheres in our tube, show any sensible absorption in the visible spectrum;

* Wied. 'Ann.,' xxxiii. p. 570.

† 'Journ. Chem. Soc.,' xxxix. p. 57.

‡ 'Comptes Rendus,' ci. p. 1144.

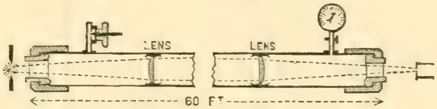
§ 'Spect. Norm.,' p. 41.

and the absorption of the ultra-violet rays by the latter gas begins at a higher point, namely about λ 2450, than that of uncombined oxygen. In fact, we see the anomalies of the selective absorption by compounds as compared with that of their elements when we take the case of water, which has a remarkable transparency for those ultra-violet rays for which oxygen is opaque.

These observations show that all stellar spectra observed in our atmosphere, irrespective of the specific ultra-violet radiation of each star, must be limited to wave-lengths not less than λ 2700, unless we can devise means to eliminate the atmospheric absorption by observations at exceedingly high altitudes.

We have extended our observations to much longer columns of oxygen. A steel tube 18 metres long (see Fig. 2) was fitted with the same quartz ends as had been used with the shorter tube, and with two quartz lenses symmetrically placed inside the tube, one near

FIG. 2.



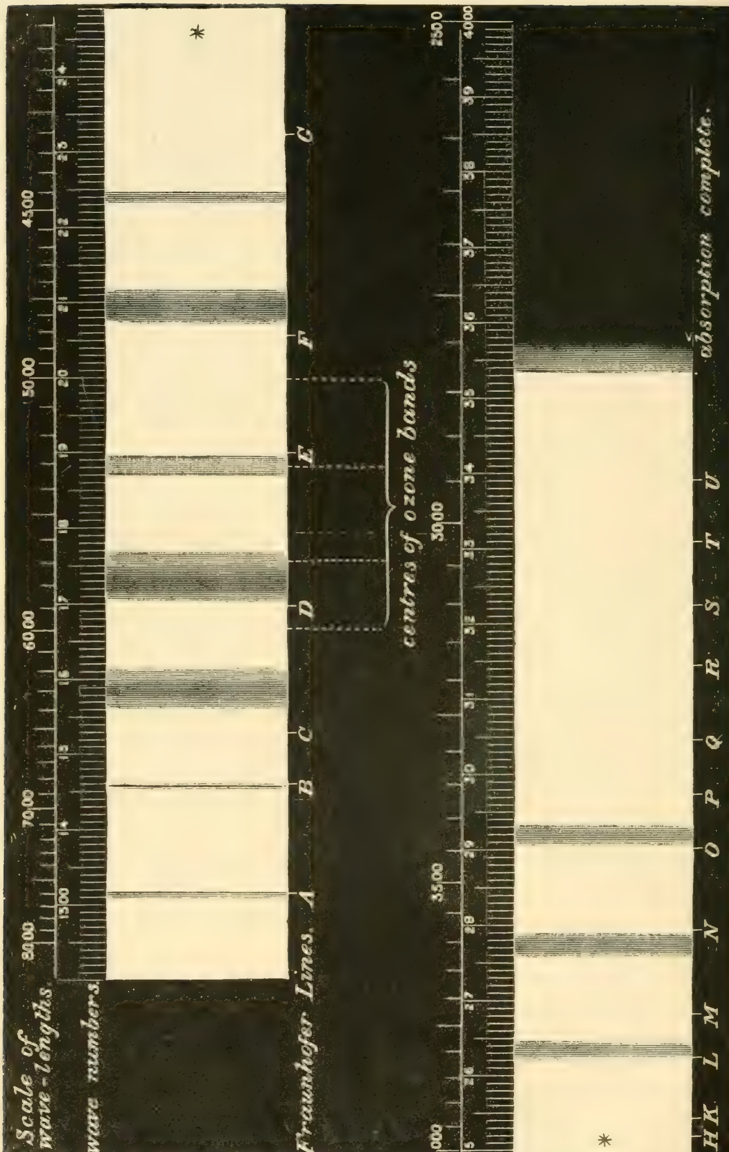
Section of Steel Tube.

each end, so that when an arc lamp was placed about 14 centimetres from one end of the tube, the image of it was formed on the slit of the spectroscope at the same distance from the other end.

When the tube was filled with air only at ordinary pressure, no absorptions could be detected, but when the air was replaced by oxygen at the pressure of the atmosphere the absorption of A was just visible, though neither B nor any other absorption-band could be traced. As the pressure of the oxygen was increased, A became much darker and more distinct, and B came out sharply defined. The absorption-band about λ 5785 was next seen, and the dark bands about λ 6300 and λ 4770, were just visible when the pressure reached 20 atmospheres.

At a pressure of 30 atmospheres A was very black, B also strong and sharply defined, and the forementioned bands were all quite strong and had the same general characters as when seen through the shorter tube; the band about λ 5350 also could be seen, but there was only a bare trace of that in the indigo about λ 4470. At 60 atmospheres these last two absorptions could be well seen, all the other bands were very strong, B still quite sharp, but A somewhat obscured by a general absorption at the red end. At 90 atmospheres this general absorption at the red end seemed to extend to about one-third of the distance between A and B; but A could still be seen, when the slit was wide, as a still darker band on a dark red background; B was still sharp, and the other absorptions all strengthened

Fig. 3.



Scale of wave-lengths.
wave numbers.
Fraunhofer Lines.
A
B
C
D
E
F
G
centres of ozone bands
H K
L
M
N
O
P
Q
R
S
T
U
absorption complete.

and somewhat expanded. The diffuse edges of several bands now extended from about—(1) λ 6410 to 6190, (2) λ 5865 to 5635, (3) λ 5350 to 5280, (4) λ 4820 to 4710, λ 4480 to 4455.

Photographs taken when the pressure of the oxygen was 90 atmospheres show a faint absorption-band about L of the solar spectrum, a stronger band extending from about λ 3600 to 3640, a broad diffuse band about the place of the solar line O, and complete absorption above P. The accompanying diagram, Fig. 3, represents the absorption of 18 metres of ordinary oxygen at a pressure of about 97 atmospheres.

The absorbent column in the tube at the highest pressure used contained a mass of oxygen about equal to that in a vertical column of the earth's atmosphere of the same section as the tube; but the intensity of the bands produced by the compressed gas was far greater than that of the corresponding bands in the solar spectrum with a low sun. When the arc light was replaced by a piece of white paper reflecting light from the sky through the tube, it appeared to the naked eye to have a faint blue tint, similar to that of liquid oxygen, which, comparing our observation with Olszewski's, seems to have the same absorptive powers as the dense gas, if we except A. This exception is probably only apparent, and due to the difficulty of observing A under the circumstances of Olszewski's experiment.

The greatly increased intensity of the absorption-bands at high pressures bears out Jansen's observation, that in this group the absorption is proportional to the product of the thickness of the absorbent stratum into the square of its density, while the absorptions to which A and B belong vary directly as the density.

The appearance, on looking through the tube when gas at high pressure is streaming into it, is very much like that of a black and a colourless liquid, which do not mix, being stirred together, and the tube soon ceases to transmit any light. Transparency returns as the density becomes uniform. Currents produced by heating the tube at one or two points produce a similar effect, and show that such currents in the atmosphere of a star may stop all rays coming from its interior.

We hope before long to get the tube fitted with rock-salt ends and lenses, and to determine the total absorption of radiation by similar masses of oxygen, nitrogen, and hydrogen.

[J. D.]

WEEKLY EVENING MEETING,

Friday, May 17, 1889.

John Rae, M.D. LL.D. F.R.S. Vice-President, in the Chair.

Professor Silvanus P. Thompson, B.A. D.Sc. *M.R.I.*

*Optical Torque.**

Seventy-eight years have elapsed since the first discovery, by Arago, of the remarkable chromatic effects produced by slices of quartz crystals upon light, previously polarised, which was caused to traverse them. These effects were shown, one year later, by Biot, to be caused by a peculiar action of the quartz in rotating the plane of polarisation; the amount of the rotation being different for lights of different colours. Ever since then, the rotation of the plane of polarisation of light has been a topic familiar to physicists. It has stimulated the devotee of research to an endless variety of experiments and suggestive speculations: it has lured on the mathematician to problems which tax his utmost skill: it has afforded to the lecturer an array of beautiful and striking illustrations. Here, in this place, made classical by the researches and expositions of Thomas Young, of Michael Faraday, and of William Spottiswoode, and last, but not least, by the labours of those eminent men whom we rejoice still to number amongst the living—here, I say, on this classic ground, the rotation of the plane of polarisation of light is almost a household word, and its phenomena are amongst the most familiar. We know now that not only certain actual crystals, such as quartz, bromate of soda, and cinnabar, rotate the plane of polarisation, but that many non-crystalline bodies—liquids, such as turpentine, oil of lemons, solutions of sugar and of various alkaloids, and even certain vapours, such as that of camphor—possess the same property.

In 1845, at the very culminating point of his unique career of research, Faraday opened a new field of inquiry, linking together for the first time the science of optics with that of magnetism, by his discovery that the rotation of the plane of polarisation of light could be effected by the application of magnetic forces. This effect he observed first in his peculiar "heavy-glass," when it lay in a powerful magnetic field. Subsequently he found other bodies to possess similar properties: some of these being magnetic liquids, such as solutions of iron, others being diamagnetic. Time will only permit me in passing to refer to the researches of Verdet, and those of Lord Rayleigh and of Mr. Gordon upon the numerical values of the

* The blocks of the woodcuts illustrating this discourse have been kindly lent by the publishers of *Nature*.

magneto-optic rotation in these substances. H. Becquerel has extended them to gases, and has shown how the magnetism of the earth rotates the plane of polarisation of the light which, previously polarised by reflection from the aërial particles which give the sky its blue tint, passes earthward through the oxygen of the air.

Other experimenters have dealt with the rotatory effects (whether crystalline, molecular, or magnetic) in relation to lights of different colours, and have studied the dispersion which arises from the greater actual angle of optical torsion which is produced upon waves of short wave-length (violet and blue) than that which is produced under the influence of equal rotatory forces upon the waves of longer wave-length (red and orange). It has also been demonstrated that the plane of polarisation of waves of invisible light, whether those of the infra-red, or those of the ultra-violet species, if they have been previously polarised, can be rotated just as can that of waves of visible light.

In 1877, Dr. Kerr, of Glasgow, discovered a point which Faraday had sought for, but fruitlessly—namely, that in the act of reflection at the pole or surface of a magnet, there is a rotation of the plane of polarisation of light. This discovery was completed in 1884 by Kundt, of Strasburg, by the further demonstration, also dimly foreseen by Faraday, that a magneto-optic rotation of the plane of polarisation is caused by the passage of previously polarised light through a normally magnetised film of iron so thin as to be transparent.

Lastly, in this brief enumeration, we were shown a month ago, by Oliver Lodge, how the magnetic impulses generated by the rapid oscillatory discharges of the Leyden jar can produce corresponding rapid oscillatory rotation in the plane of polarisation of the waves of previously polarised light.

You will not have failed to notice the cumbrous phrase which, whether in speaking of the purely optical effects (of quartz, or sugar, or turpentine), or in speaking of the magneto-optic effects of more recent discovery, I have employed to connote a very simple fact. You may have wondered that any lover of simple English speech should indulge in such sesquipedalian words.

Of course, at this period of the nineteenth century it is no longer open to debate that light consists of waves. The plane of polarisation of the waves of light is the plane of polarisation of the light itself. The rotation of the plane of polarisation is the rotation of the polarised waves, and therefore of the polarised light itself. Yet I must draw attention to the fact that in all the array of discoveries which I have enumerated, that which had been observed was the rotation—whether by crystalline, molecular, or magnetic means—not of natural light, but of light which had by some means been previously polarised. It was not known to Arago or to Biot, to Fresnel, to Faraday, nor even to Spottiswoode or to Maxwell, that natural unpolarised light could be rotated. They may have inferred so, but it was not in their time even demonstrable that a beam of circularly-

polarised light could be rotated upon itself in the same sense as that in which a beam of plane-polarised light can be rotated.

That light of any and every kind, however completely polarised or devoid of that which is called polarisation, can be, and in fact is, rotated when it passes across a slice of quartz or along a magnetic field, is a wider generalisation of more recent date; but one of the reality of which I hope to convince you before the warning finger of the clock puts a period to my discourse.

In order the better to enable this audience to comprehend the ultimate significance of this discovery, I must claim the indulgence of those amongst them who are already familiar with the subject of the polarisation of light, whilst I go back to the most simple elementary matters. Having illustrated the fundamental facts about the plane of polarisation of light and its twisting, I shall then go on to methods of precisely measuring the amount of optical torsion produced by the various substances under various conditions. And after dealing with the magnetic as well as the crystalline and molecular methods of producing optical torsion in the case of light that has been previously polarised into a given plane, I shall be in a position to speak of the nature of the torque,* or twisting force, which in the several cases produces the torsion; and shall finally endeavour to indicate the scope of the researches by which it is now definitely ascertained that the very same optical forces which are capable of impressing a rotation upon light which has been artificially polarised into a definite plane, are also capable of impressing a rotation upon natural non-polarised light.

At the outset, to elucidate to any who may not comprehend the meaning of the term polarisation as applied to wave-motion, I will show a simple apparatus, constructed from my designs by Mr. Groves. In this there are two sets of movable beads, fixed upon stems which pass into a box containing a piece of mechanism actuated by means of a handle. These beads, when I turn the handle, oscillate to and fro in definite directions, and, by their successive motions, give rise to progressive waves. One set of beads, tinted red, executes movements in a plane inclined 45° to the right, another set, silvered, simultaneously executes movements at 45° to the left. There are therefore here two waves, the planes of polarisation of their movements being at right angles to one another. Their velocity of march is equal; but in this model, as a matter of fact, their phases differ by one-quarter—that is to say, each successive wave of the one set is always a quarter of a wave-length behind the corresponding wave of the other set. [Model exhibited.]

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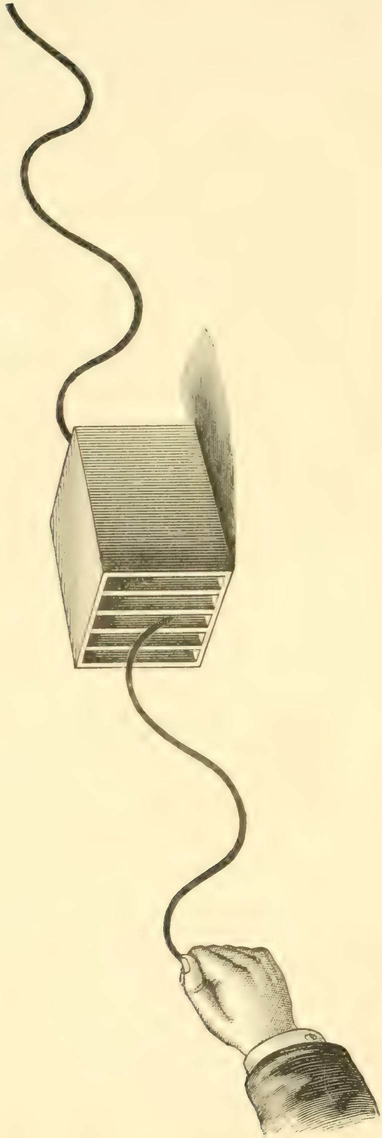
* The convenient term *Torque* was first proposed by Prof. James Thomson, of Glasgow, for the older and more cumbrous phrase "moment of couple," or "angular force." Its general acceptance by engineers justifies the extension of the term to optics. As a mechanical torque is that which produces or tends to produce mechanical torsion, so optical torque may be defined as that which produces or tends to produce optical torsion.

Now, in the case of waves of natural light from all ordinary sources—sun, stars, candles, gas-flames, or electric lights—the waves emitted are not found to be polarised. That is to say, their motions are not executed in any particular plane, nor even in any particular path of any kind; they appear to be absolutely heterogeneous at least so far as this, that no vibration of the millions of millions emitted in a second of time is followed by more (on the average) than about 50,000 vibrations of a similar sort, executed along a similar path—the plane of the polarisation, if any, changing after the lapse of such an incredibly short time that for most purposes the vibrations in different directions are as inextricably mixed as if they had all been simultaneously jumbled up. Since, then, natural light is non-polarised or miscellaneous, the production of polarised light must be brought about by the employment of polarising apparatus or agents which will so operate on or affect the mixed waves as to bring their vibrations into one direction—or, what amounts to the same thing, transmit the light whilst destroying or absorbing those parts of the vibrations which are executed *across* the desired line of vibration. So we have *polarisers* consisting of tourmaline slices; oblique bundles of thin glass plates; black-glass reflectors; and Nicol prisms cut from calc-spar. About the two latter I may be permitted a passing word presently. These objects polarise, i. e. turn into one plane, the vibrations of light falling upon them. A rough mechanical illustration may here be permitted me. A long indiarubber cord is passed through the open ends of a box provided with vertical partitions. Fig. 1 shows the arrangement. These partitions confine the motion of the cord, and effectually polarise the vibrations which I now impart to the cord by shaking the end of it to and fro. If the partitions are vertical, the box polarises, into vertical vibrations only, the miscellaneous vibrations which are sent to it. If rotated until its partitions are horizontal, it polarises the vibrations into a horizontal position.

Let us now turn to the optical analogue of this experiment. The large Nicol prism which I introduce into the field of the electric-light lantern, polarises the light, so that the vibrations are executed simply in an up-and-down direction. Your eye will not detect this, the motion being millions of times too rapid. To detect the direction an analyser is necessary. For this purpose a second apparatus of the same sort is used, for then, by crossing the positions of the two, the whole of the light is cut off; the second Nicol prism, if set so as to transmit only horizontal vibrations, cutting off the vertical vibrations that are sent through the first prism. So, while the first prism serves as a polariser, the second serves as an analyser to detect by cutting them off when turned to the proper position, the direction of the polarisation which had been previously impressed by the first prism.

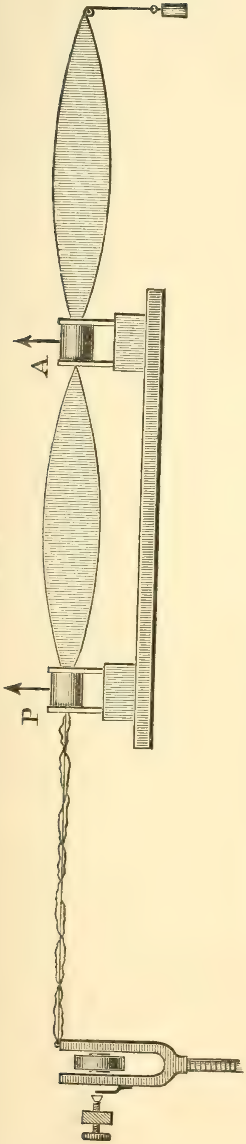
Here I may illustrate the action of the analyser for determining the plane of polarisation of the vibrations by the extinction which it produces when turned to the crossed position. For this purpose I

Fig. 1.



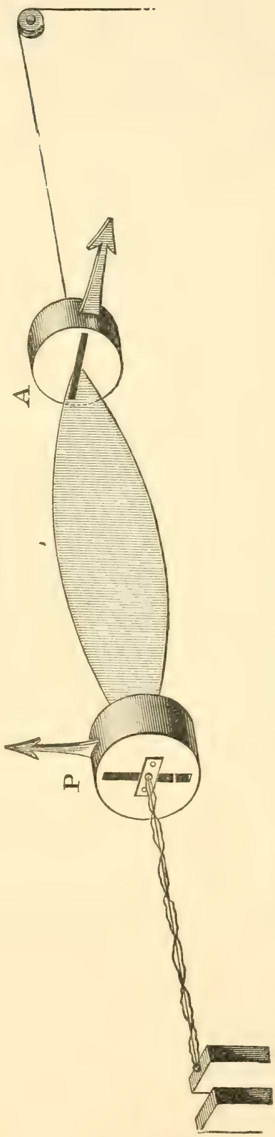
Box with partitions to illustrate polarisation of vibrations.

Fig. 2.



Acoustic model illustrating polarisation of vibrations. P, the polariser; A, the analyser.

Fig. 3.



Vibrations cut off by turning the analyser A at right angles to the polariser P.

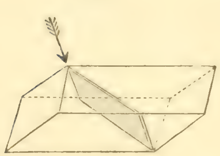
have refined upon the box with partitions, using instead parallel plates of glass mounted in wooden cylinders, whilst for the cord swung by hand I am using Professor Schwedoff's device, and am producing the vibrations in this silken cord by means of an electrically-driven tuning-fork (Fig. 2). At the first nodal point of the stretched cord a pair of parallel glass plates act as a polariser, the cord beyond that point vibrating in the plane thus imposed upon it. I can alter this plane at will by rotating the polariser. This polariser, P, consisting of a pair of glass plates, is mounted in a cylindrical mount, and is provided with an arrow to indicate their direction. If now at any subsequent node I introduce a second such device, it will act as an analyser, A. This excellent suggestion is due to M. Macé de Lepinay. In Fig. 2 the polariser and analyser are parallel. You see (Fig. 3) how the vibration is extinguished when the positions of analyser and polariser are crossed. Half a degree of error in the position of the analyser produces something less than perfect extinction of the vibrations. Hence it is possible, by this analyser, to determine the plane of the vibrations to the accuracy of half a degree. I should say that the whole of this model has been constructed by my assistant Mr. Eustace Thomas.

Now let me show you the optical effect which corresponds to this. Placing a second Nicol prism as analyser in the path of the polarised waves, I turn it to the position where it cuts off the polarised light. The "*dark field*" so produced by the crossed Nicol prisms corresponds to the motionless cord beyond the crossed analyser of the acoustic apparatus.

Returning for a moment to two well-known forms of polarising apparatus, viz. the black glass reflector and the Nicol prism, I may be permitted to refer to some recent attempts to improve upon these devices.

The Nicol prism, as is well known, consists of a rhomb of Iceland spar cut into two pieces, which are reunited by a film of Canada balsam. As originally devised, it had oblique end faces (Fig. 4) and a comparatively narrow angle (19°) of aperture. These may be noticed in the small example which I here exhibit, which is an original constructed by William Nicol himself. It also has the disadvantage of giving a field in which the directions of the planes of polarisation are not strictly parallel to one another throughout its whole extent. Consequently there is never complete extinction of light all over the field at one time. Hartnack and others have attempted to remedy this by giving the prism a different form and using other materials than Canada balsam. I have from time to time made many attempts to improve upon the original construction.

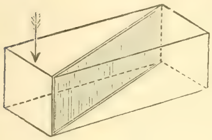
Fig. 4.



Nicol prism: original form: arrow shows crystallographic axis.

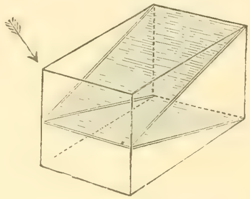
First, I have made the end faces principal planes of section (Fig. 5); secondly I have made the axis of vision cross the crystallographic axis at right angles, so getting a flatter field, a shorter length, a wider angle, and less loss of light by reflection. Mr. Ahrens, the prism-cutter, on whose able assistance I have relied during the last six or seven years in cutting these prisms, has aided me with his ingenuity in devising a method of cutting up the spar so as to give these advantages with a minimum waste of material. He has further devised a method of putting a polarising prism together in three

FIG. 5.



S. P. Thompson's modification of the Nicol prism.

FIG. 6.



Ahrens's triple prism.

instead of two pieces—illustrated in the diagram (Fig. 6)—which gives a still wider angle. The prism which I shall use as analyser in the next experiments is one of these forms.

Unfortunately at present there is a spar-famine, pieces of Iceland spar of a size and purity suitable for the making of large polarisers such as that I employ being not now procurable at any price. To avoid the excessive cost of large Nicols I have lately got Mr. Ahrens to construct for me a large reflection-polariser, on the plan of Delezenne, but modified by Mr. Ahrens in detail. In this prism the light is first turned to the proper polarising angle by a large total-reflection prism of glass, and then reflected back, parallel to its original path, by impinging upon a mirror of black glass covered by a single sheet of the thinnest patent plate glass to increase the intensity of the light. This form of polariser, depicted in Fig. 7, is quite equal for projection purposes to a Nicol prism of equal aperture, and is much less costly. This one has $2\frac{7}{8}$ inches clear aperture.

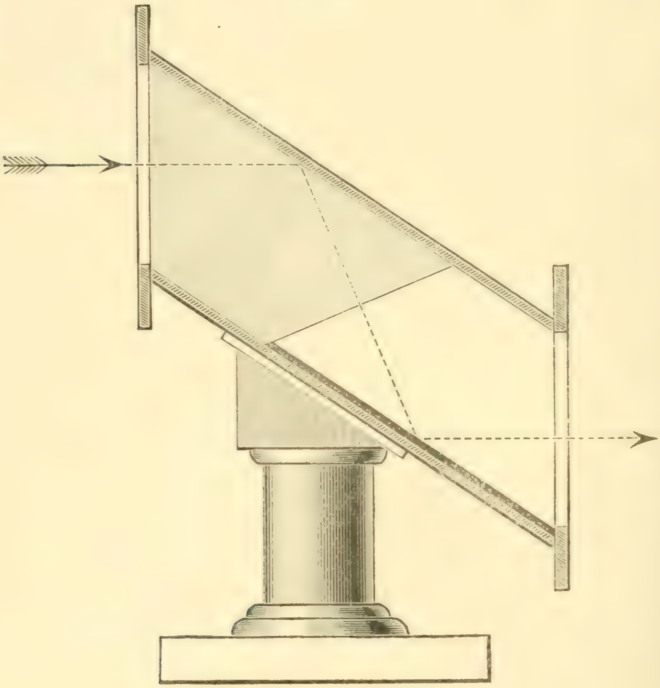
Having so far reviewed the apparatus for polarising and analysing I will return to the apparatus set with its prisms crossed, so that the analyser completely extinguishes the polarised light emitted from the polariser.

If in the space between polariser and analyser anything be introduced which can either resolve obliquely the polarised vibrations or twist them bodily round, then there will not be complete extinction; the amount of light passing the analyser depending in the one case on the obliquity of the resolution, in the other upon the degree to which the vibrations are twisted or rotated upon themselves.

The effect of oblique resolution I may illustrate by introducing a slice of tourmaline between the crossed Nicols, and rotating it till it stands at 45° ; or, in the acoustic model, by introducing an oblique pair of guide pins.

The other case—namely, that of producing a bodily twist of the vibrations, rotating the plane of polarisation around the path of the wave—is not so easily illustrated by the model. But it is optically perfectly simple: all that is requisite is to introduce between the

FIG. 7.



Ahrens's reflecting polariser.

crossed Nicols a thin slice of that crystal—namely, quartz—in which this effect of rotating the plane of polarisation was first observed.

I take a clear plate of quartz, just 1 millimetre in thickness, and interpose it between the crossed Nicol prisms. You will note how the introduction of this plate of quartz brings *some* light into view.

Suppose we now turn the analyser to try and obtain extinction: we get tinting. If we put in a coloured glass so as to work with one kind of light only, we shall get extinction at a particular angle. The

table of data to which I invite your attention states this amount for the different colours.

Optical Torsion Produced by Plate of Quartz.

	1 millimetre.	3·75 millimetres.
Red	19°	71°·2
Orange	21·5	80·6
Yellow	24	90
Green	29	108·7
Peacock	31	116·2
Blue	35·5	133·1
Violet	42·8	161

If we use a piece of quartz so thick that it rotates any particular tint just 90°, that tint will be cut off by the crossed analyser, and all others will—in greater or less proportion—be transmitted, so that the resulting tint will be complementary to that cut off. For example, a slice so thick as to twist yellow waves round 90° must be just 3·75 millimetres thick. (I may remark, for the benefit of those who think it easier to express this exact thickness in fractions of a British inch, that the quartz which rotates yellow light 90° must have a thickness equal to one-eighth, plus three-sixteenths of an eighth, plus one sixty-fourth of an eighth of an inch.) When such a quartz is placed between the crossed Nicols the light shown is yellow; but if placed between parallel Nicols (i. e. in the bright field) it shows a rich purplish-violet colour, the complementary of the yellow. This particular tint Biot found to be excessively sensitive, the smallest inaccuracy in adjustment between the prisms at once producing a change, the colour appearing too red or too blue, according to the directions in which the analyser has been turned out of exact adjustment. This tint is accordingly known as the "transition tint" or "sensitive tint," its accurate definition being due to the fact that the human eye is more sensitive to the presence or absence of the complementary yellow than to any other tint in the whole spectrum. If we take, however, a quartz plate twice as thick as this—namely, 7½ millimetres thick—it will give the yellow light a torsion of 180°. Hence this thickness gives the purple transition tint in the dark field, and yellow in the bright field. A quartz plate 11¼ millimetres thick gives again a transition tint in the bright field. I shall recur presently to the question of the transition tints of the several orders.

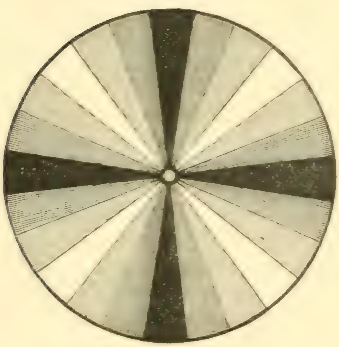
One of the familiar facts in this subject is that there are two kinds of quartz crystals, optically alike in every other respect, differing only in this, that one kind produces a right-handed twist, the other kind a left-handed twist. All the pieces of quartz I have so far employed are right-handed specimens. I now introduce two small slices of crystal, each 3·75 millimetres thick, giving the yellow tint when the Nicols are exactly crossed, but you will notice that when we are using the right-handed crystal, the tint grows reddish as the

analyser is turned towards the left, and greenish when the analyser is turned towards the right; whereas, when I substitute the left-handed slice, the tint grows greenish as the analyser is turned towards the left, and reddish when it is turned towards the right. If the analyser is turned through an exact right-angle, we get an extinction of the yellow light, the remaining blue and red rays combining to give us the purple transition tint.

You will have noticed that the way in which we have (approximately) measured the angle of rotation has been first to set the analyser to extinction, then to introduce the substance which has the property of rotating the beam, then to turn the analyser again to extinction, and read off its angle. For, of course, the angle through which the analyser is turned measures the angle through which the plane of polarisation has been turned.

It is possible, however, to show in the lantern something like a more obvious rotation of the light by introducing between the Nicols a crystal star, built up of radial pieces of mica, twenty-four in number (Fig. 8). You see in the bright field a white cross with black sectors at 45°. Or, in the dark field we have a black cross with vertical and horizontal arms, the sectors next to those that are black seeming dusky. If now I put in a quartz plate between the star and the analyser, you see the cross shift round, and it shows colours, because the blue rays are twisted round more than the green, the green than the yellow, the yellow than the red. Repeating the experiment with the 3·75 millimetre quartz which turns yellow waves round just 90°, we get this gorgeous radiation of colours, and our black cross is turned into a yellow one. With the 7·5 millimetre quartz, the black cross is replaced by one of "transition" tint.

FIG. 8.



Mica disk of twenty-four rays, showing black cross in the dark field.

The black crosses seen in certain sections of natural crystals, sphæroliths, sections of stalactites, crystallisations of salicine and of Epsom salts, may also be used instead of the 24-rayed star of mica. But best of all I find to be the beautiful black cross which is seen by polarised light in the prepared crystalline lens taken from the eye of a fish. You notice how, when the fish-lens is projected and the quartz introduced, the cross turns round.

This is, however, a rough-and-ready way of displaying the rotation, and it is of vast practical importance that precise methods of measuring the angle of rotation should be available—of vast importance,

because in several large industries this optical process is applied as a method of rapid analysis. I have named a solution of sugar as being an "active" substance. In the industry of sugar-refining, as in that of brewing, the strength of sugar in the liquids is directly measured by measuring its optical effect. Consequently there has been developed a special instrument, the *polarimeter*, for this express purpose.

I have here examples of several practical forms of polarimeters; there are diagrams of several more upon the walls.

The problem of finding the best polarimeter naturally leads to the inquiry what special means there are for making the observation of the angle more precise than by merely observing the extinction of the light, its restoration when the active substance is interposed, and the subsequent renewal of extinction when the analysing prism is turned.

Biot considered that much greater accuracy could be attained by watching for the restoration of the sensitive tint than by watching for the mere restoration of extinction of the light. Accordingly we will use the plate of quartz 7·5 millimetres thick, giving the purple tint, to enable us to measure the rotation produced by the tube of sugar solution which is now inserted in the beam of polarised light. You notice how the tint has changed. But I have only to turn the analyser to an amount equal to that to which the light has been twisted by the sugar, and again I obtain the sensitive transition tint.

The eye is not always, however, alive to minute changes of colour in a single coloured patch; it much more readily distinguishes a minute difference between two tints when both are present at once. Hence Soleil devised the well-known biquartz arrangement, consisting of two pieces of crystal, equal in thickness, but possessing opposite rotations. You will notice how the slightest inaccuracy in placing the analyser causes the two halves of the field to differ in tint. This is especially marked when the tint chosen is the transition purple.

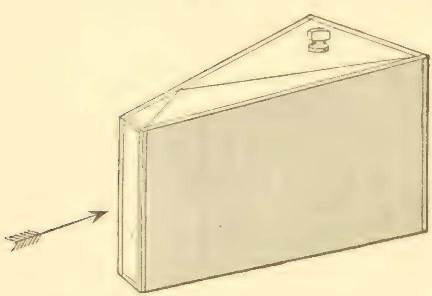
It will be convenient here for me to refer to some researches, not yet published, which I have made, as to the various orders of transition tints, with the view of ascertaining which of them is the most sensitive —which of them, in fact, shows the greatest change of tint for the smallest amount of rotation. Reference to the diagram on the wall displaying Newton's tints will make clear what I mean by the transition tints of the several orders. The tints obtained from quartzes of varying thicknesses may be considered as approximately identical with the tints of Newton's rings, provided we remember that the air-film which gives any particular tint in Newton's rings is about 1/300,000 part as thick as the quartz which yields the corresponding tint in the polariscope. Better far than any painted diagram, because richer and purer, are the tints now thrown upon the screen by introducing into the field a thin wedge of selenite, displaying the whole of the colours of the first three orders of Newton's scale. You will notice the successive recurrence of purple tints, both in the colours seen in the bright field, and in those seen in the dark field.

First I will show you the transition tints of the first and second orders in the bright field. That of the second order is much less intense than that of the first; and yet it is very sensitive, turning to a green tint whilst the first order purple has only turned to a blue. On the other hand, with reversed rotation of the analyser it turns to red less rapidly than does the tint of the first order.

Next I take the transition tints of orders I., II., and III. in the dark field. These, though arranged, by means of superposed half-disks of "quarter-wave" plates, to be optically equivalent to biquartzes of two rotations, are really built up of selenite and mica. You will notice how the tint of order I. surpasses in sensitiveness both the others. I cannot here show you on the screen the means by which I have compared the tint of order I. in the dark field with that of order I. in the other set. Suffice it to say that I find the tint of order I. in the dark field—corresponding to 7·5 millimetres thickness—more sensitive than that of order I. in the bright field, which corresponds to 3·75 millimetres thickness.

A method which was at one time supposed to be more precise, was that of placing a spectroscope (or its prism) in front of the analyser, and watching the motion along the spectrum of the interference bands which are then seen. My three pieces of crystal remain. I introduce a slit in front of them, also a single film of quarter-wave mica, and then a prism to give the spectrum. This prism (Fig. 9), by the way, is a new sort of direct-vision prism, having a single very wide-angled prism of Jena glass inclosed in a cell with parallel ends containing cinnamic ether (first recommended by Wernicke), a liquid which has the same mean refractive power but widely different dispersion. It is preferable to bisulphide of carbon in several respects; its odour is a delicate reminiscence of cinnamon; it is barely volatile; and it is whiter than bisulphide. This prism, which is shown also in plan in Fig. 10, was constructed for me by Messrs. R. and J. Beck. It will be seen that the dark bands in the spectrum are nebulous and ill-defined. It was proposed to secure accuracy by turning the analyser until they shift along to a definite point. But their want of definition prevents precision. There is no advantage in using the higher orders of tints which give more bands; for, though the bands are certainly better defined, their progression across the spectrum for a given amount of rotation is proportionally smaller.

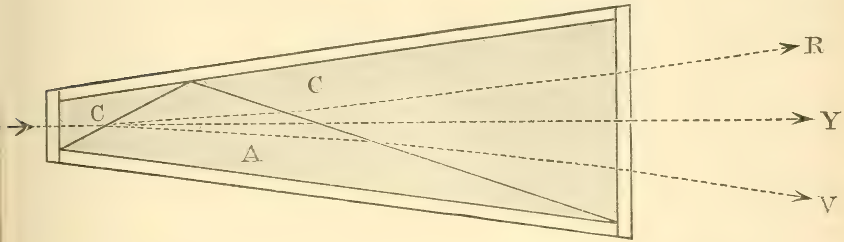
FIG. 9.



Direct-vision prism for projection of spectrum.

Another suggestion, due to Sénarmont, is to use two sets of superposed wedges of right- and left-handed quartz. Such you now see before you. Instead of starting with extinction you start with coinci-

FIG. 10.



Direct-vision prism. A, wide-angled prism of Jena glass; C, cinnamic ether.

dence between the upper and lower set of bands. Any rotation of the light shifts the bands, one set moving to left, the other to right. By turning the analyser through an equal angle coincidence is again obtained.

Another method, used by Wild in his polaristrobometer, is to produce the phenomenon known as Savart's bands (due to the introduction of two crossed slices of quartz cut at a particular angle). The bands disappear when the analyser is set in a particular direction. Anything that twists the plane of polarisation causes them to reappear; but they again fade out when the analyser is turned through an equal angle.

There is another method in exact polarimetry, due to Soleil, in which the optical torsion due to the sugar is counterbalanced or compensated by introducing a pair of sliding wedges of quartz of the opposite rotation. This device is known as a "compensator." By sliding the quartzes over one another a greater or less thickness of quartz is introduced at will. But I must not stop to illustrate this elegant device.

Yet one other method must be mentioned, and this is certainly the most preferable. It consists in aiding the eye to recognise with precision a particular degree of extinction, by the device, first suggested in 1856 by Pohl, of covering a portion of the visible field with something which slightly alters the initial plane of polarisation, so that complete blackness is not obtained at once over both parts of the field. A common device is to cover half the field with a slice of some thin crystal—mica or quartz—so that only one half can be perfectly black at any instant. As an example, here is the field covered half over with a plate of mica of the thickness known as half-wave. The result is that when one half of the field is black the other is light. Adjust the analyser now to equality. Now introduce something that

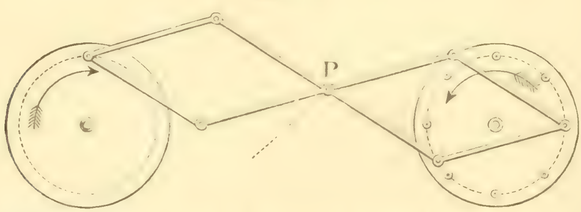
rotates the light—say a tube with sugar solution in it. At once the balance is upset, and I must, in order to get equality of illumination, turn my analyser through an angle equal to that of the optical torsion.

Of the same class are the polarimeters with special prisms made in two parts slightly inclined to one another. The earliest of these was devised by the late Professor Jellett, of Dublin, and has been followed by imitations of the same plan by Cornu, by Lippich, and by Schmidt and Haensch. The beautiful "shadow polarimeter," by the latter firm, which I here exhibit, has the divided prism, and a quartz compensator.

I have suggested two simpler methods of accomplishing the same end. In the first place, I have proposed to use *twin-prisms*. These are made on a plan suggested to me by finding that Mr. Ahrens's method of cutting calc-spar for prisms was admirably adapted for making such prisms, either with wide or narrow angles between the respective planes of polarisation in the two parts of the visible field. Two such twin-prisms, one with 90°, the other with $2\frac{1}{2}$°, between the prisms, are here on the table. In the second place, I have essayed a polarimeter, an example of which is before you, in which an arrangement of twin-mirrors (each set at the polarising angle, but slightly inclined to one another) is made to yield a half-shadow effect.

Before I leave the subject of quartz I must refer to the famous mathematical theory of Fresnel, who endeavoured to explain its action

FIG. 11.



Model illustrating recomposition of rectilinear motion from two opposite circular motions.

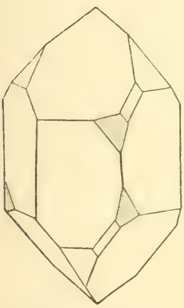
upon light by supposing that the plane-polarised wave on entering it is split into two waves, consisting of oppositely circularly-polarised light, which traverse the crystal with different speeds. On emerging they recombine to form plane-polarised light, the plane of which, however, depends on the retardation of phase between the two components. I here introduce a mechanical model to illustrate one of the points in this theory—namely, the recombination of two circular motions to form a straight-line motion. These two disks (Fig. 11), which turn in opposite senses, but at equal rates, represent two circularly-polarised beams of light. The linkages, which connect two pins on these disks, compound their motions at the central point, P, which executes, as

you see, a straight line. But now, suppose one of these circular motions to be retarded behind the other, an effect which I can imitate by shifting one of the pins to another position on the disk. Still the resultant motion is a straight line, but it is now executed in a direction oblique to the former. In other words, its plane has been rotated. Of course this model must not be taken as establishing the truth of Fresnel's ingenious theory; it is at best a rough kinematical representation of it.

We have, however, the puzzling fact still to account for that there should be two kinds of quartz crystals, right- and left-handed. Sir John Herschel first showed that natural crystals of quartz themselves often indicated their optical nature, by the presence of certain little secondary faces or facets which lay obliquely across the corners of the primary faces. These are indicated in the diagrams (Figs. 12 and 13), and may be seen in two of the specimens of quartz crystals

FIG. 12.

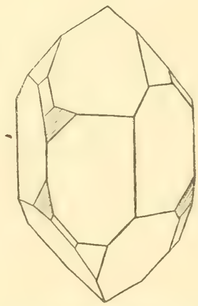
R



Quartz crystal, showing characteristic facets: right-handed.

FIG. 13.

L

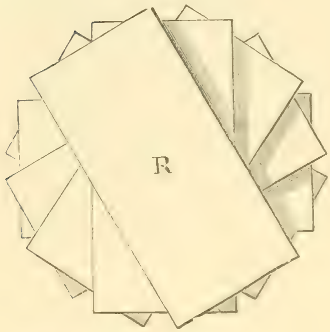


Quartz crystal, showing characteristic facets: left-handed.

which lie upon the table. The largest of these is right-handed. The wider generalisations of Pasteur, respecting the crystalline form of optically active substances, show that those substances which exercise an optical torque, whether as crystals or in solution, belong to the class of forms which the crystallographer distinguishes as possessing non-superposable hemihedry. In other words, they all show *skew symmetry*, as if in the growth of them they had been built up in some screw-fashion around an axis, and must therefore be either right-handed or left-handed screws. By piling up a number of wooden slabs in skew-symmetric fashion, I am able roughly to illustrate (Figs. 14 and 15) the difference between the right-handed and the left-handed structure. It is a curious fact, if I am rightly informed, that down to the present date the only substances possessing this skew symmetry are natural substances; that those which the chemist can produce by artificial synthesis are all optically inactive. It is per-

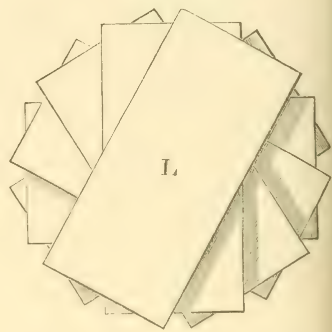
haps equally significant that as yet no inorganic substances have been found which will in the liquid state rotate the light. This appears to be a property possessed solely by certain compounds of carbon.

FIG. 14.



Skew-symmetrical arrangement: right-handed.

FIG. 15.



Skew-symmetrical arrangement: left-handed.

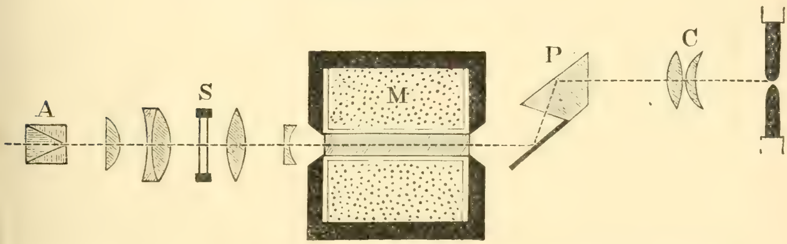
Quartz fused in the blowpipe or dissolved in potash shows no trace of rotatory power.

Yet we can have little doubt that this property is bound up in the yet unravelled facts of atomic and molecular structure. In the case of the liquids, such as turpentine, and sugar solution, there must be some skew symmetry in the grouping of atoms in the molecule to produce the result. In the case of quartz, there must be a skew in the building of the molecules; there must—to borrow a phrase from the architect—be an oblique *bonding* of the minute bricks of which its transparent mass is builded. Though we cannot even rebuild it from its solution, we know this must be so, for we can reproduce all the optical phenomena which it exhibits by an actual skew building of thin slices of another non-rotatory crystal. Here is an artificial object (I built it myself) constructed on Reusch's plan, from sixteen thin slips of mica built up in staircase fashion—right-handedly—one above the other, and set symmetrically at equal angles of 45° to one another, the whole set making a cork-screw of two complete turns. In the lantern it behaves just as a quartz of about 9 millimetres thickness would do. It even gives tolerably perfect rings, as quartz does, when viewed by convergent light.

I must now pass hastily onwards to the great discovery of Faraday. Here (Fig 16) is a magnetising coil of wire M, having about 8300 turns, and enclosed in an iron jacket. When it is traversed by a powerful electric current from the dynamo machine, it produces an intense magnetic field along its axis. In this axial position lies a bar of heavy glass, not quite so dense as that which Faraday himself used,

but nearly so. The bar lies along the line of light from our lantern, but the polariser P (the Ahrens reflector, Fig. 7), and analyser A (the Ahrens triple spar prism, Fig. 6), are crossed, so that here is the dark field. On turning on the current, light is at once restored, being twisted to the right when the current circulates right-handedly. To measure the rotation, I must turn the analyser; and now I find that, owing to the greater rotation of blue waves than of red, complete extinction does not occur. Introducing a half-shadow plate, and using coloured glasses, it is very easy to verify the greater amount of rotation for blue light, and to show that reversing the current reverses the rotation. You will perhaps better understand it if I use (as in Fig. 16) the 24-ray star S, which I have previously employed. It is now obvious to you that there is a large rotation—over 50° in fact—which is reversed when I reverse the magnetising current. We have here the fundamental experiment of magneto-optics. But now we meet with another consideration. Reflect that the circulation of current, if it be taken as right-handed when regarded from one end of the coil, will be left-handed when regarded from the other end of the coil.

Fig. 16.



›jection of magnetic rotation of light. C, condensing lenses; P, reflecting polariser; ıagnetising coil surrounding bar of heavy glass; S, mica star of twenty-four rays; A, analyser (Ahrens's triple prism).

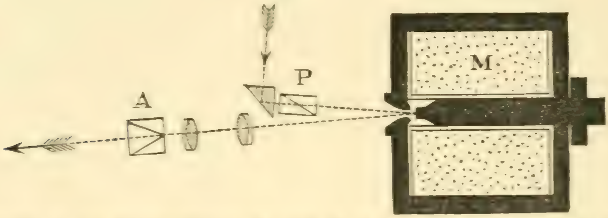
his is, therefore, no case of skew symmetry; it clearly indicates that ›mething is going on in the glass which tends to twist the light quite respective of which way the light enters.

The next magneto-optic phenomenon is that discovered by Dr. Kerr the rotation of the plane of polarisation by reflection at the surface 'a magnet. To observe this at all requires good apparatus and a ›en eye. So far as I am aware, it has never been projected on the reen. If I can succeed in doing so, it will only be because I have ›ecial means of the most favourable character for so doing. We ithdraw the bar of heavy glass from the coil, and replace it 'ig. 17) by an iron core polished at its coned end. This will be tensely magnetised when the current is turned on.

Now we must throw the beam of light obliquely down the hollow the coil, polarising it by one of my improved Nicol prisms P, as

it goes down. After reflection it is focussed by a lens which sends it through the analysing prism A. You see the dim spot of reflected

FIG. 17.



Apparatus for projecting rotation of plane of polarisation by reflection at pole of magnet. P, polariser; M, magnetising coil with coned iron core; A, analyser.

light upon the screen. Now for the current: "on," "off," "on." "off." Reversing its direction ought to double the amount of torsion.

Whilst Mr. Thomas is making the needful arrangements for the next experiment, I may mention that it was found by Kerr that the effect was approximately proportional to the magnetic induction through the iron. I have myself tried some further experiments: for example, using a bar of lodestone instead of an iron core. The light reflected from lodestone is also twisted. I should expect the ferro-aluminium alloy which Sir H. Roscoe showed us a fortnight ago to do the same thing, because that alloy is, as I have found, susceptible of magnetisation. But I should not expect manganese steel to rotate the light, because of its singularly non-magnetisable nature.

The experiment of Kundt, transmitting polarised light through a thin transparent film of iron, magnetised normally whilst the light is passing through it, is another difficult of repetition before an audience. The small disks here are covered with films of iron, kindly prepared for me by Mr. Crookes, by squirting them electrically in a high vacuum. But the thin ones barely transmit enough light to make the observation of the effect possible, even to the solitary observer. I have observed the effect projected on the screen, using this very coil and these transparent mirrors. It requires, however, an absolutely dark room, and is at best so faint that it would be hopeless to attempt to show it to a large audience. Professor Kundt has not only observed similar rotations in other magnetic films of nickel and cobalt but has even shown that the degree of rotation of the light is proportional not to the magnetising force, but to the resulting magnetic induction. This is a result of utmost importance in considering the theory of the phenomenon. He has further shown that whereas the magnetic rotations in elementary bodies, whether magnetic or diamagnetic, are in the same sense as that in which the current circulates, the magnetic rotations in compound magnetic bodies, such as a solution of sulphate of iron in water, are in the opposite sense.

These experiments with transparent mirrors of iron raise interesting speculations as to the probable nature of a transparent magnet, if such there could be. It is one of the cardinal points of Maxwell's celebrated electro-magnetic theory of light, that the better a body conducts electric currents, the greater is its tendency to absorb light and become opaque. Now, suppose it were possible to obtain a substance such as to possess greater electric conductivity in one direction than in another, such a substance ought to absorb those vibrations of light which are executed in the direction of the greater electric conductivity more than those in the direction at right angles. In other words, such a substance ought, like the tourmaline, to polarise light by absorption. Now, since the researches of Sir W. Thomson in 1856, we have known that the electric conductivity of iron is altered in the direction of the magnetic lines of force, when it is powerfully magnetised. More recently it has been discovered—I myself observed it in tinfoil, and announced the discovery to the Physical Society a few days before the announcement of the same fact by Righi—that non-magnetic metals alter their resistance in the magnetic field. Notably so do bismuth and tellurium. I had therefore conceived it possible that a film of iron or possibly of tellurium, if strongly magnetised in its own plane, might exhibit polar absorption and act like a tourmaline. Unfortunately, if the effect exists it is so faint as to be yet undiscovered, though I have made many efforts to find such. I have further tried to obtain a similar result by making a transparent magnet out of a film of magnetic oxide of iron, precipitated chemically. In this too I have not succeeded. I have tried to precipitate a transparent film of magnetic oxide in the midst of a transparent jelly. And I have mixed particles of precipitated oxide with melted gelatine so as to get a film. In this way I hoped to get, by placing the preparation in a strong magnetic field, a sort of magnetic structure which would operate upon waves of light. That such a task was not hopeless was shown by two facts: first, that many mere vegetable and animal structures can act as polarisers; and second, that a mere film of paint, such as indigo, can, if a proper mechanical drag is given to it so as to produce structure, also act as a polariser.

The film of indigo-carmine which I have here, acts nearly as strongly, though not quite as evenly, as a tourmaline slice, and costs but a fraction of a penny.

Well, my films of jelly enclosing particles of magnetic oxide of iron do faintly act on polarised light; but their action is not as marked as that of films of jelly enclosing actual small scraps of iron. This film, when placed across the poles of this electromagnet, between two crossed Nicols at 45°, shows an action when the magnet is turned on, as you see by the way in which it flashes into light in the dark field. When the jelly is fresh, and of the proper consistency, the action is very strong, but with the rather dry sample before you I fear we can only call the effect a *succès d'estime*.

Incidentally, in the course of these experiments on magnetic films,

I came across a new magnetic body unknown hitherto, I believe, to the chemist, namely, a magnetic double oxide of cobalt and iron—a ferroso-cobaltic oxide, I think—a black powder, a sample of which I have here.

It also occurred to me, as a matter of speculation, that if I could strongly magnetise a crystal of ferrous sulphate or nickelous sulphate, whilst viewing it by convergent polarised light, I might find some interesting phenomena, which should, if they existed, show some sort of a relation between the direction of the optic axis and that of the lines of the magnetic field. I thought that a longitudinal magnetisation might possibly set up a rotatory phenomenon like that in quartz in so far as to disturb the central field between the arms of the black cross; however, not by the most powerful magnetising could I discover any such effect. Again, I thought that by magnetising transversely to the optic axis I might possibly succeed in turning the uniaxial crystal into a biaxial, or producing by magnetism an effect resembling the action of heat on crystals of selenite. Owing probably to the small depth of any crystals that can be obtained, I have failed so far to obtain any such effect, though I am convinced that it must exist.

An effect precisely analogous to the magnetic effect which I vainly sought has, however, been lately discovered by Prof. Röntgen. I sought a distortion of the optic axis by transversely magnetising, and I sought it in crystals of sulphate of nickel; he has found a distortion of the optic axis by transversely electrifying, and he has found it in crystals of quartz.

Suppose a piece of quartz crystal is cut as a square prism, its long faces being principal planes of section respectively parallel to and at right angles to two of the natural faces of the hexagonal prism. Fig. 18 shows the form of the portion cut. The + and – signs in this

Fig. 18.

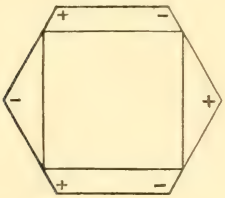


Fig. 19.

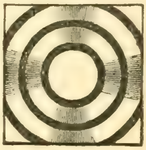


figure refer to the pyro-electric poles of the crystal. Such a piece viewed by convergent light shows the usual rings and black cross with a coloured centre (Fig. 19). If now two opposite faces be covered with tinfoil, and the crystal be electrified transversely, the rings are distorted into lemniscates, the direction of the distortion changing with the sign of the electrification. It is necessary to use a red

glass, or still better sodium light, to observe the changes in form on reversing the sign of the charges. Figs. 20 and 21, 22 and 23, show the changes of form, but these sketches grossly exaggerate the effects. As you see upon the screen, when the charges imparted by this fine Wimshurst machine are rapidly reversed, there is a decided distortion of the rings, but it is small in amount.

Fig. 20.

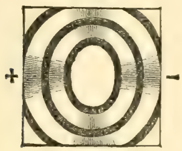


Fig. 21.

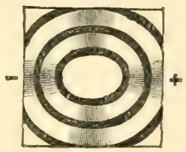


Fig. 22.

Fig. 23.

Returning to the phenomena of the rotation impressed by magnetism on polarised light, I may point out that the torque which a magnetic field exerts on the light waves appears to be really an action upon the matter through which the light-waves are passing. It is as though the magnetic field were really a portion of space rotating rapidly on itself, or perhaps as though the ether were there rotating, and that this rotation in some way dragged the particles of matter along with it. It has long been supposed necessary, in order to account for the refractive and dispersive properties of transparent bodies, to consider that their particles are in some way concerned in and partake of the vibrations going on in the ether within them or between their molecules. It is impossible to explain the phenomena of magneto-optic rotation by the supposition that any skew structure is imparted to the medium; for these phenomena unlike those of quartz, do not exhibit skew symmetry. There seems to be no other way of explaining the magneto-optic torsion of light than by supposing that the molecules of matter in the magnetic field are actually subjected to rotatory actions; as indeed was suggested long ago by Sir William Thomson.

However, there is room here not only for speculation but for experiment. Some day, when facts enough have been collected, we shall

be ready to build thereon the wider generalisation which at present seems to escape us.

So far we have been applying an optical torque to previously polarised light, and producing a torsion of it. It remains for me yet to describe the means by which, in the hands of Professor Abbe and Professor Sohncke, it has been demonstrated that natural, non-polarised light is actually rotated when subjected to an optical torque.

The way of doing this is to make use of the principle of interference. Here is the slit from which a narrow beam of light-waves issues. At a point a little distance away is a Fresnel's biprism which splits up the light (without polarising it) into two beams, just as if we had two slits or sources of light. These two beams pass along, and meet upon this distant screen, and give us—what? A set of interference fringes having a bright line down the middle, because this part of the screen is exactly equidistant from the two sources of light.

But these dark interference fringes that lie right and left can only exist because, in the first place the vibrations have travelled unequal paths differing by an odd number of half wave-lengths; and secondly, because (owing to the method adopted of using two images of one slit) the phases of the emitted waves from the two sources are identical.

This being so, let us now introduce across the two interfering beams of light a special biquartz, made of right-and-left handed quartz of only 1·88 mm. thick. This will rotate—*if it rotates natural light at all*—the yellow light in one beam 45° to the right and that of the other beam 45° to the left. The angles will be a little more for green and blue, a little less for red and orange. Consequently we shall not get quite a perfect result for all kinds of colours. But for the main body of the light the result is this: that because the two beams have had their respective vibrations turned so that, whatever their primitive positions, they are now at right angles to one another, they cannot interfere. In other words, if it be true that the quartz rotates natural light, the interference bands will die out. [Experiment shown.]

Here I have the light passing through the biprism only, and giving us this narrow series of interference bands. You must notice carefully—with opera-glasses if you have them—the narrow bright and dark stripes. Now I shift this little diaphragm so that the light passes through the biquartz as well. Instead of sharp interference bands we have merely a dull line of nebulous light. The disappearance of the fringes proves that quartz does twist the non-previously polarised waves of light.

That the magnetic field can also exert a magnetic torque on non-polarised light is readily proved, at least when one already has the biquartz. Two strips of heavy glass of exactly equal length and similar quality, such as those I hold in my hand, must be introduced in the respective paths of the two beams: and one at least of them must be surrounded by a magnetising coil. The biquartz has wiped

out the interference fringes; but on magnetising one of the two pieces of heavy-glass, or on magnetising the two in opposite senses, the interference bands can be made to reappear. It is in this way that Professor Sohncke's experiment—hardly suitable for a lecture theatre—was performed. It is in this way that we establish upon an experimental basis the fact that light itself, and not merely the plane of its polarisation, experiences an optical torsion when subjected to those forces which, whether crystalline, molecular, or magnetic, exert upon it an optical torque.

[S. P. T.]

WEEKLY EVENING MEETING,

Friday, May 24, 1889.

Sir James Crichton Browne, M.D. LL D. F.R.S. Vice-President, in the Chair.

The Rev. S. J. Perry, D.Sc. F.R.S. F.R.A.S. Director of Stonyhurst College Observatory.

The Solar Surface during the Last Ten Years.

The solar surface is a subject on which so much has been written of late years, that it would be highly unsatisfactory to attempt in one short hour even a brief enumeration of the results obtained and the theories advanced by the many eminent men who have devoted attention to this branch of solar physics. The end which I propose to myself this evening, and which, I venture to think, is most in accordance with precedent in these discourses, is to lay before you, in as clear a manner as I am able, the results obtained at the observatory to which I am attached, in so far as they enter into our present subject, and to touch upon the work of others in such a manner only as to complete the picture, by showing the bearing of our labours on the genêral result.

For the last ten years I have been anxiously endeavouring to make Stonyhurst as efficient an observatory for solar physics as the means at my disposal would admit, so I was naturally desirous not to undertake any work that would be a mere repetition of what was being done better elsewhere. From the outset, therefore, I excluded from my programme a daily photograph of the sun, as this had been already undertaken at the Royal Observatory, assisted by other Government observatories in India and at Mauritius, and the most I could have hoped for in this direction would have been to fill up a few gaps in an almost perfect series. My choice, therefore, lay between drawings made at the telescope by aid of a solar eye-piece, and the use of a sketch-board on which the sun's image could be projected. The object in view being to procure the most complete and faithful representation of nature, I had no hesitation in rejecting all forms of solar eye-pieces, as by this method of observation too much is left to the imagination of the draughtsman; although a solar prism is not unfrequently of great advantage in supplementing other methods of attack, especially when delicate details require verification. The sketch-board, on the other hand, may be so used as to leave very little indeed to the ideas or bias of the observer. To effect this the following method has been adopted. A circle ten and a half inches in

diameter is first traced upon a piece of drawing paper, which is pinned to a board just rigid enough to hold the paper firmly, and then the whole is clamped on to the eye-end of the telescope. The eye-piece and board are each capable of fine adjustment, so that the sharpest image of the sun may be made just to fill the $10\frac{1}{2}$-inch circle, and a marked diameter of the picture is brought into precise coincidence with the direction of the daily motion. The clock-work of the equatorial then keeps the image fixed in position on the paper, whilst an accurate outline is traced of the umbra and penumbra of every spot visible on the disk. When the sky is clear this outline can be made as correct as the finest point of a hard pencil can delineate it; and even when, as so often happens, the transparency of the atmosphere is changing every moment, a short time at the instrument will generally enable the observer to verify the perfect accuracy of his sketch. The details are then filled in as quickly as the nature of the sky permits, each portion of the drawing being over and over again brought into coincidence with the projected image, in order to detect and remove the slightest difference between them. By this means the final picture gives the advantage of all the best moments of seeing that occur during the progress of the observation, and not merely the result at one single moment, which may be far from the best for definition even on the finest day. Immediately the spots have been completed the faculæ are traced, and their details reproduced as nearly as possible. By the advice of Prof. Stokes, P.R.S., a red pencil is used in drawing the faculæ, and thus the difference between bright and dark markings and their connection with each other, stand out much more boldly than if the same black pencil were used throughout. Finally, when the sky is good for definition, the general surface of the sun is carefully scrutinised for some time, and any peculiarities noted.

The sun's image having thus been sketched and examined, the drawing-board is replaced by an automatic spectroscope of six prisms of 60°, through each of which the light may be made to pass twice, and to which may be added a Hilger-Christie half-prism, raising the total available dispersion to about 36 prisms of 60°. The chromosphere is measured with the slit radial, a dispersion of six prisms being generally used; and when the definition is very good, a sweep is made round the limb, with 12 prisms and a tangential slit, to study the forms of the prominences and the direction of the currents in the chromosphere. Spot-spectra have been occasionally observed with the same instrument; but lately (in a room adjoining the equatorial dome) a large grating has been mounted, with which it is proposed to take daily photographs and eye-measurements of the spectra of sun-spots. A heliostat and a $5\frac{1}{2}$-inch object-glass of Alvan Clark are used in connection with the grating spectroscope. The combination of this solar observatory with an establishment supplied with a complete set of self-recording meteorological and magnetic instruments, affords a ready opportunity of comparing solar results with the daily photographic records of terrestrial phenomena.

As it is most important, before referring to any conclusions that may be drawn from our observations, to test severely the fidelity of which solar drawings are capable, I will throw upon the screen a number of sun-pictures drawn at Stonyhurst, and enable you to contrast them with sketches of the same spots made by experienced astronomers in England, and at Brussels, Palermo, and Kalocsa, and also with photographs taken at Dehra Dun and at Meudon; and I think these few examples will amply suffice to vindicate a high character for solar drawings. But I have not been satisfied with this ready comparison, and lately this solar work has been put to a more rigid test of accuracy by placing side by side the measurements of areas obtained from drawings and photographs; and here again the hand-sketch by projection seems to bear well the scrutiny.

The method of projection not only permits the area covered by spots and faculæ to be accurately determined, but it also furnishes precise data for finding the heliographic co-ordinates of any mark upon the solar surface. These, however, are now given so fully in the annual publications of the Royal Observatory, and each individual spot can be so readily identified, that an independent calculation would be a mere waste of time and energy. The results, therefore, dependent on position alone, are deduced immediately from the Greenwich Tables.

The decade of years which we are now considering, covers almost an entire solar cycle, that period of eleven years the proof of whose existence was the fruit of the unwearied labours of Baron Schwabe, in his daily observations from 1826 to 1868. The present cycle falls much below that which preceded it in the extent of its spotted area, but it is remarkable for the duration of its maximum period. The last minimum occurred about November 1878, and therefore, if we accept the mean values $3 \cdot 7$ and $7 \cdot 4$ as the number of years from minimum to maximum and from maximum to minimum respectively, we obtain $1878 \cdot 9 + 3 \cdot 7 = 1882 \cdot 6$ as the date of the maximum of the cycle, and $1882 \cdot 6 + 7 \cdot 4 = 1890 \cdot 0$ as the epoch of the approaching minimum. Many facts seem to support this conclusion. Thus, the greatest sun-spot area occurred on April 21, 1882, and the largest individual spot was at its maximum on November 18 of the same year, when it covered almost one four-hundredth of the visible hemisphere. The total spot area in April reached, however, the much higher figure of one one-hundred-and-eightieth, or about 6,000,000,000 square miles, which on the following day had diminished to something under 4,000,000,000, showing the marvellous activity of the solar forces at that epoch. In the same year also the mean monthly amount of umbra was greater in April and in November than at any other time of the cycle, making it highly probable that the disturbances then penetrated more deeply below the surface of the photosphere. Again, if we consider the mean latitude of the spotted area, its value at the maximum seems also to favour the claims of 1882; and these are

still more strengthened by the number of times in which the spectral lines have been found contorted in the observations of the chromosphere. But, on the other hand, it may be urged, that if we reckon the days without spots, we find very little change from 1880 to the beginning of 1885; and the monthly mean area of sun-spots was slightly in excess in July 1883 of what it had been in the previous year; and even as late as June 1885 we meet with days on which the spotted area was larger than the mean extent for any month in 1882 or 1883. May it not be possible that the great comet of 1882, which passed so deep within the limits of the corona, and which seems to be but one of a numerous family, may have exercised some disturbing influence, and been a partial cause of this prolonged maximum?

The limits are rather wide for the periods separating maximum from maximum, but there is much more steadiness in the lapse of time between successive minima, and if we may judge from the present rapid increase in the number of days without spots, the next minimum should be fast approaching, and may not be far removed from the computed epoch of January 1890. Thus, against one spotless day in 1884, we have 10 for 1885, 61 for 1886, 106 for 1887, 160 for 1888, and more than half the days that have been fit for observation since the beginning of the present year. The important law connecting the mean latitude of sun-spots with their extent of area, first published by Carrington, and afterwards so vividly represented in curves by Prof. Spörer, has received a fresh verification in the present cycle. In close relation to this is the distribution of spots in the two hemispheres, and this can be well illustrated by aid of curves and by mapping each spot in its true heliographic position. Thus we find that the greater activity of the southern hemisphere since 1883 is strongly marked, and there seems to be very little evidence that successive spots tend to form along meridians, although examples of this are not wholly wanting. A general glance at spot distribution shows that an outbreak may be expected on any meridian, but that at certain times spots congregate more thickly in one longitude than in another. Thus in 1881 and 1882 the longitudes most favoured lie between 180° and 270°, whilst in 1887 more spots appeared between 0° and 90°. The preponderance of southern spots in this year is very striking.

The variability of the sun-spot area which we have been so far considering, is a point of the greatest interest in solar physics, and it may even become of great practical importance, if the supposed connection between certain terrestrial phenomena and solar cycles is once clearly established. But apart from this, the study of spots in themselves, their wonderful changes, and their individual history, may be highly instructive and teach us much concerning the nature of the solar surface. The spots seen first in March and April 1884, and which reappeared in May and June and July, furnish excellent examples of the formation of companion groups, the principal spot acquiring a

regularity of outline when it stands alone in middle life, and regaining new companions as it hurries on to its final extinction.

The last few years have also afforded some striking instances of the repulsion and proper motion of spots, so clearly brought out in the researches of Carrington. Perhaps the most remarkable change of this nature recorded at Stonyhurst occurred between May 27 and 28 in 1884, when a spot of considerable size, situated between two others, moved through a great part of the distance separating its two companions.

Another class of observations consists in noting the variety of tints connected with the bridges and other bright portions of intricate spots, and which are seen only from time to time. Thus, in 1884, on the 5th of May and the 6th of July, there were outbursts of a crimson hue in the centre of spots. Such appearances vary in duration as much as in colour. On one occasion a reddish brown tint, very noticeable on the bright separations of the umbra of a spot, did not remain constantly visible, but was intermittent. At another date a yellowish green tint remained visible for several days on the bridge crossing a spot. In each case the coloured portion was carefully compared with other parts of the disk, so as to ascertain whether the effect might not be due to the object-glass or eye-piece.

Some useful hints in view of future theory can certainly be gained by a careful study of the peculiar forms assumed occasionally by the penumbra of spots, not when the whole area is in a state of violent commotion, for then it is conceivable that any form may possibly present itself, but when the umbra is regular and the spot quiet. I would especially draw attention to some examples of multiple penumbræ observed in 1882 and 1883, which seem to indicate that such disturbances are due to successive impulses from a common centre. Or, again, I would point to the penumbral matter extending from the nucleus of certain spots in 1884, which might reasonably be adduced as evidence of an apparent outflow of a floating substance. It is doubtless fascinating to argue in support of some exhaustive theory, complete in itself, and able alone to offer a satisfactory explanation of every observed appearance; but may it not be possible that several true causes concur in the production of solar phenomena? And if this be admitted, then a line too hard and fast may easily stand in the way of the true explanation of spot formation. Might not the last mentioned observations suggest the question, whether absorbent vapours may not sometimes be cast up from the seething mass beneath, although a down-rush be the prevailing feature of a sun-spot?

The study of the bright markings on the solar surface is perhaps somewhat less interesting, although almost equally important as that of the spot area, but the state of the sky interferes much more seriously with the former class of observation than with the latter. The clustering of faculæ about nascent spots, their spreading as the spots gets older, their lingering long after the parent spots have

disappeared, the apparition of fresh spots in their midst, are facts of constant recurrence. That faculæ are the first evidence of a coming disturbance has never been observed at Stonyhurst, but a region where faculæ abound has always been found to have previously been occupied by a group of spots. Such an observation as that of the birth of the great November spot of 1882, and other similar instances are not wanting, in which a few dots, in a region of perfect calm, suddenly developed into a vast centre of disturbance, is a strong proof that the law which regards faculæ as the constant forerunners of spots, cannot be accepted as universal. Do not such instances as these give some additional weight to the opinion that large spots owe their origin rather to an external cause, as the prime mover, than to the internal forces of heat and pressure and chemical affinity? Might not the latter forces, however great, hold each other almost in equilibrium until their energies are freed by the advent of an external, though perhaps lesser, disturbing force? The periodicity of sun-spots, their total absence for months, nay almost even for years, combined with their enormous development and rapid changes about the epoch of maximum area, seems to preclude the possibility of internal forces, in a gaseous body, being the sole cause of such phenomena; although there appears to be no reason why these forces should not exercise an overwhelming influence when called into play by an external agent.

The distribution of faculæ, especially of small isolated patches, is much more general than that of spots, many being visible even near the sun's poles, and this has been forced upon my attention more and more of late as we have approached nearer the minimum of the cycle.

Another point, which is often accepted as established, is the lagging of faculæ, and it is sometimes adduced as a proof that faculæ are cast up from a lower level, for thus, in possessing a less linear velocity than the surrounding photosphere, they would naturally be left behind. The observations of the present cycle can scarcely be said to add to the stringency of this argument, as out of more than 4000 cases recorded in the Greenwich Tables, 74 per cent. lean neither way, and of the remainder 5·7 per cent. show the faculæ preceding, against 20·3 per cent., which alone are in direct confirmation of the assumed law.

The general surface, with its ever varying aspect, can never be adequately represented by the pencil, but in this case recourse must be had to the instantaneous photograph. And yet it is true to say that even in this branch of the subject much may be done by the method of projection, and light may thus be thrown on many points which could scarcely be settled by photography alone. I refer particularly to any possible variation of tint in definite regions of the solar disk, to rapid and continuous changes, and to such appearances as the smugged areas in the photographs, which suggest at first bad definition, but which are in reality evidence of a new form of disturbance not indicated by other phenomena. Watching the image of

the sun as it depicts itself upon the drawing-board, we soon perceive that no part of the surface is long at rest. Everywhere are seen small dark shadowy objects, attracting special attention now in one place and then in another, sometimes forming groups, and at other times almost companionless, affecting no special zone, and combining with no other form of solar marking, except occasionally with a few bright faculæ.

These dim ill-defined objects seem first to have been observed in 1875 by Trouvelot, who gave them the name of veiled spots. They soon forced themselves into notice when our daily sun-work was started at Stonyhurst, and ever since they have never failed to be carefully watched. At first they were divided, for convenience, into three classes, but on further examination there seems to be no need of more than a single distinction. When they first catch the eye all present much the same general appearance, resembling small fragments of ill-defined penumbra, but their position on the disk, and still more their duration, soon enable the observer to distinguish the class to which they belong. Those of the first class appear in all heliographic latitudes, and never remain visible for more than two or three minutes ; whilst the others, which have been called sub-permanent spots, are confined exclusively to the spot zones on either side of the equator, and this class may remain on the disk for two or more days. Sub-permanent spots are not always to be found on the surface of the sun, their tint is a shade less dull than that of the other veiled spots, and occasionally there is almost the appearance of an umbra in their midst, though this rarely could be mistaken for a true umbra. For even when their shading is in some parts more intense than in others, the whole remains always ill-defined, and the limits of its several parts are hard to distinguish. Frequently these dim objects show themselves in considerable numbers in the neighbourhood of fully developed spots, but then the latter are generally approaching their time of dissolution. They may, perhaps, aptly be described as imperfectly developed, or penumbral spots, and consequently be included in the ordinary spotted area. But it is quite otherwise with the first class of veiled spots, which except for their diminished brilliancy would have nothing in common with the fully developed sun-spots. Seen in all solar latitudes, they are never absent from the sun, being, with good definition, as frequent and as visible at the epoch of spot maximum as at minimum, but catching the eye more readily when markings more intense are absent from the surface. The most striking characteristic of this class of spots is the rapidity with which they invariably disappear ; but although no individual spot ever lasts more than about three minutes, the first seen may be joined in quick succession by a multitude of others similar to itself, and thus transform vast areas, and give to portions of the solar surface that blurred appearance which is so marked a feature in Janssen's magnificent sun-pictures. The general distribution of these faint objects, their evanescent character, and their ill-defined appearance,

seem to connect them more immediately than any other feature of the solar surface with the vertical convection currents which form so important a part of the sun's internal economy.

One exceptional observation of the formation of veiled spots may be of interest, as showing the strange phenomena we may occasionally witness by a lengthened examination of the solar image. At 10h. 15m. one morning a group of spots was visible on the sun, and presented no very special feature; less than half an hour later the leader of the group had apparently shot out a number of minute bodies, and then five minutes sufficed for all these to be transformed into veiled spots, which disappeared as rapidly as usual.

Instances have not been wanting of other moving bodies seen upon the solar surface, always rapid in their course, and sometimes disappearing without crossing the limb. I refer, of course, only to observations in which every precaution was taken to test the objective nature of these bodies, as false images might so easily deceive a tired retina, especially after exposure to a strong light. But there would be little use in dwelling upon even well-established cases of this nature, as they all probably find their explanation in the passage of bodies between the sun and the observer, and promise very little additional light for an inquiry into the nature of the solar photosphere.

Before concluding these few words on a series of solar observations of the last ten years, I may be expected by some to add one more to the already long list of solar theories, or at least to pass in rapid review the most plausible theories that up to the present time have been advanced for reducing to harmony all the well-established facts regarding the sun with which we are acquainted. But considering the scope of my discourse, which has dwelt mostly on the work done at a single observatory, and, even so, has only treated in detail one method of examining the solar photosphere, I should not be warranted in advancing any theory of my own, or in judging of the theories of others, without first considering the many important facts connected with our subject which the spectroscope has taught us and also extending my researches to the solar chromosphere and the corona. It is only by diligently collating the facts laboriously accumulated by telescope and spectroscope in every known region of the sun that we may hope at last to build up a complete and satisfactory solar theory. Much has already been achieved in this direction, and we are in possession not only of reliable data, but also of important deductions therefrom, which may serve as a solid foundation for some future superstructure.

I may have added, perhaps, some little towards the completion of the edifice, if I have convinced you this evening, that by the persevering and judicious use of the pencil we may yet hope to throw some light on questions of solar physics, that might not so readily have been secured by any other means. [S. J. P.]

WEEKLY EVENING MEETING,

Friday, May 31, 1889.

Sir Frederick Abel, C.B. D.C.L. F.R.S. Vice-President, in the Chair.

D. Mendeléeff, Esq. LL.D.

PROFESSOR OF CHEMISTRY IN THE UNIVERSITY OF ST. PETERSBURG.

An Attempt to apply to Chemistry one of the Principles of Newton's Natural Philosophy.

Nature, inert to the eyes of the ancients, has been revealed to us as full of life and activity. The conviction that motion pervaded all things, which was first realised with respect to the stellar universe, has now extended to the unseen world of atoms. No sooner had the human understanding denied to the earth a fixed position and launched it along its path in space, than it was sought to fix immovably the sun and the stars. But astronomy has demonstrated that the sun moves with unswerving regularity through the star-set universe at the rate of about 50 kilometres per second. Among the so-called fixed stars are now discerned manifold changes and various orders of movement. Light, heat, electricity—like sound—have been proved to be modes of motion; to the realisation of this fact modern science is indebted for powers which have been used with such brilliant success, and which have been expounded so clearly at this lecture table by Faraday and by his successors. As in the imagination of Dante, the invisible air became peopled with spiritual beings, so before the eyes of earnest investigators, and especially before those of Clerk Maxwell, the invisible mass of gases became peopled with particles: their rapid movements, their collisions, and impacts became so manifest that it seemed almost possible to count the impacts and determine many of the peculiarities or laws of their collisions. The fact of the existence of these invisible motions may at once be made apparent by demonstrating the difference in the rate of diffusion through porous bodies of the light and rapidly moving atoms of hydrogen and the heavier and more sluggish particles of air. Within the masses of liquid and of solid bodies we have been forced to acknowledge the existence of persistent though limited motion of their ultimate particles, for otherwise it would be impossible to explain, for example, the celebrated experiments of Graham on diffusion through liquid and colloidal substances. If there were, in our times, no belief in the molecular motion in solid bodies, could the famous Spring have hoped to attain any result by mixing carefully dried powders of potash, saltpetre, and acetate of soda, in order to produce, by pressure, a chemical reaction between these substances through the interchange

of their metals, and have derived, for the conviction of the incredulous, a mixture of two hygroscopic though solid salts—nitrate of soda and acetate of potash ?

In these invisible and apparently chaotic movements, reaching from the stars to the minutest atoms, there reigns, however, a harmonious order which is commonly mistaken for complete rest, but which is really a consequence of the conservation of that dynamic equilibrium which was first discerned by the genius of Newton, and which has been traced by his successors in the detailed analysis of the particular consequences of the great generalisation, namely, relative immovability in the midst of universal and active movement.

But the unseen world of chemical changes is closely analogous to the visible world of the heavenly bodies, since our atoms form distinct portions of an invisible world, as planets, satellites, and comets form distinct portions of the astronomer's universe ; our atoms may therefore be compared to the solar systems, or to the systems of double or of single stars, for example, ammonia (NH^3) may be represented in the simplest manner by supposing the sun nitrogen surrounded by its planets of hydrogen ; and common salt (NaCl) may be looked upon as a double star formed of nitrogen and chlorine. Besides, now that the indestructibility of the elements has been acknowledged, chemical changes cannot otherwise be explained than as changes of motion, and the production by chemical reactions of galvanic currents, of light, of heat, of pressure, or of steam power, demonstrate visibly that the processes of chemical reaction are inevitably connected with enormous though unseen displacements, originating in the movements of atoms in molecules. Astronomers and natural philosophers, in studying the visible motions of the heavenly bodies and of matter on the earth, have understood and have estimated the value of this store of energy. But the chemist has had to pursue a contrary course. Observing in the physical and mechanical phenomena which accompany chemical reactions the quantity of energy manifested by the atoms and molecules, he is constrained to acknowledge that within the molecules there exist atoms in motion, endowed with an energy which, like matter itself, is neither being created nor is capable of being destroyed. Therefore, in chemistry, we must seek dynamic equilibrium not only between the molecules but also in their midst among their component atoms. Many conditions of such equilibrium have been determined, but much remains to be done, and it is not uncommon, even in these days, to find that some chemists forget that there is the possibility of motion in the interior of molecules, and therefore represent them as being in a condition of death-like inactivity.

Chemical combinations take place with so much ease and rapidity ; possess so many special characteristics, and are so numerous, that their simplicity and order was for a long time hid from investigators. Sympathy, relationship, all the caprices or all the fancifulness of human intercourse, seemed to have found complete analogies, in

chemical combinations, but with this difference, that the characteristics of the material substances—such as silver, for example, or of any other body—remain unchanged in every subdivision from the largest masses to the smallest particles, and consequently their characteristics must be a property of its particles. But the world of heavenly luminaries appeared equally fanciful at man's first acquaintance with it, so much so, that the astrologers imagined a connection between the individualities of men and the conjunctions of planets. Thanks to the genius of Lavoisier and of Dalton, man has been able, in the unseen world of chemical combinations, to recognise laws of the same simple order as those which Copernicus and Kepler proved to exist in the planetary universe. Man discovered, and continues every hour to discover *what* remains unchanged in chemical evolution, and *how* changes take place in combinations of the unchangeable. He has learned to predict, not only what possible combinations may take place, but also the very existence of atoms of unknown elementary bodies, and has besides succeeded in making innumerable practical applications of his knowledge to the great advantage of his race, and has accomplished this notwithstanding that notions of sympathy and affinity still preserve a strong vitality in science. At present we cannot apply Newton's principles to chemistry, because the soil is only being now prepared. The invisible world of chemical atoms is still waiting for the creator of chemical mechanics. For him our age is collecting a mass of materials, the inductions of well-digested facts, and many-sided inferences similar to those which existed for Astronomy and Mechanics in the days of Newton. It is well also to remember that Newton devoted much time to chemical experiments, and while considering questions of celestial mechanics, persistently kept in view the mutual action of those infinitely small worlds which are concerned in chemical evolutions. For this reason, and also to maintain the unity of laws, it seems to me that we must, in the first instance, seek to harmonise the various phases of contemporary chemical theories with the immortal principles of the Newtonian natural philosophy, and so hasten the advent of true chemical mechanics. Let the above considerations serve as my justification for the attempt which I propose to make to act as a champion of the universality of the Newtonian principles, which I believe are competent to embrace every phenomenon in the universe, from the rotation of the fixed stars, to the interchanges of chemical atoms.

In the first place I consider it indispensable to bear in mind that, up to quite recent times, only a one-sided affinity has been recognised in chemical reactions. Thus, for example, from the circumstance that red-hot iron decomposes water with the evolution of hydrogen, it was concluded that oxygen had a greater affinity for iron than for hydrogen. But hydrogen, in presence of red-hot iron scale, appropriates its oxygen and forms water, whence an exactly opposite conclusion may be formed.

During the last ten years a gradual, scarcely perceptible, but

most important change has taken place in the views, and consequently in the researches of chemists. They have sought everywhere, and have always found systems of conservation or dynamic equilibrium substantially similar to those which natural philosophers have long since discovered in the visible world, and in virtue of which the position of the heavenly bodies in the universe is determined. There where one-sided affinities only were at first detected, not only secondary or lateral ones have been found, but even those which are diametrically opposite, yet among these, dynamical equilibrium establishes itself not by excluding one or other of the forces, but regulating them all. So the chemist finds in the flame of the blast furnace, in the formation of every salt, and, with especial clearness, in double salts, and in the crystallisation of solutions, not a fight ending in the victory of one side, as used to be supposed, but the conjunction of forces; the peace of dynamic equilibrium resulting from the action of many forces and affinities. Carbonaceous matters, for example, burn at the expense of the oxygen of the air, yielding a quantity of heat and forming products of combustion, in which it was thought that the affinities of the oxygen with the combustible elements were satisfied. But it appeared that the heat of combustion was competent to decompose these products, to dissociate the oxygen from the combustible elements, and therefore, to explain combustion fully, it is necessary to take into account the equilibrium between opposite reactions, between those which evolve, and those which absorb heat.

In the same way, in the case of the solution of common salt in water, it is necessary to take into account, on the one hand, the formation of compound particles generated by the combination of salt with water, and on the other the disintegration or scattering of the new particles formed, as well as of those originally contained. At present we find two currents of thought, apparently antagonistic to each other, dominating the study of solutions: according to the one, solution seems a mere act of building up or association; according to the other, it is only dissociation or disintegration. The truth lies, evidently, between these views; it lies, as I have endeavoured to prove by my investigations into aqueous solutions, in the dynamic equilibrium of particles tending to combine and also to fall asunder. The large majority of chemical reactions which appeared to act victoriously along one line have been proved capable of acting as victoriously even along an exactly opposite line. Elements which utterly decline to combine directly may often be formed into comparatively stable compounds by indirect means, as for example in the case of chlorine and carbon; and consequently the sympathies and antipathies, which it was thought to transfer from human relations to those of atoms, should be laid aside until the mechanism of chemical relations is explained. Let us remember, however, that chlorine, which does not form with carbon the chloride of carbon, is strongly absorbed, or, as it were, dissolved by carbon, which leads us to suspect incipient chemical action even in an external and purely

surface contact, and involuntarily gives rise to conceptions of that unity of the forces of nature which has been so energetically insisted on by Sir William Grove and formulated in his famous paradox. Grove noticed that platinum, when fused in the oxyhydrogen flame, during which operation water is formed, when allowed to drop into water decomposes the latter and produces the explosive oxyhydrogen mixture. The explanation of this paradox, as of many others which arose during the period of chemical renaissance has led, in our time, to the promulgation by Henri St. Claire Deville of the conception of dissociation and of equilibrium, and has recalled the teaching of Berthollet, which, notwithstanding its brilliant confirmation by Heinrich Rose and Dr. Gladstone, had not, up to that period, been included in received chemical views.

Chemical equilibrium in general, and dissociation in particular, are now being so fully worked out in detail, and applied in such various ways, that I do not allude to them to develop, but only use them as examples by which to indicate the correctness of a tendency to regard chemical combinations from points of view differing from those expressed by the term hitherto appropriated to define chemical forces, namely, "affinity." Chemical equilibria dissociation, the speed of chemical reactions, thermo-chemistry, spectroscopy, and, more than all, the determination of the influence of masses and the search for a connection between the properties and weights of atoms and molecules; in one word, the vast mass of the most important chemical researches of the present day, clearly indicates the near approach of the time when chemical doctrines will submit fully and completely to the doctrine which was first announced in the Principia of Newton.

In order that the application of these principles may bear fruit it is evidently insufficient to assume that statical equilibrium reigns alone in chemical systems or chemical molecules: it is necessary to grasp the conditions of possible states of dynamical equilibria, and to apply to them kinetic principles. Numerous considerations compel us to renounce the idea of statical equilibrium in molecules, and the recent yet strongly supported appeals to dynamic principles constitute, in my opinion, the foundation of the modern teaching relating to atomicity, or the valency of the elements, which usually forms the basis of investigations into organic or carbon compounds.

This teaching has led to brilliant explanations of very many chemical relations and to cases of isomerism, or the difference in the properties of substances having the same composition. It has been so fruitful in its many applications and in the foreshadowing of remote consequences, especially respecting carbon compounds, that it is impossible to deny its claims to be ranked as a great achievement of chemical science. Its practical application to the synthesis of many substances of the most complicated composition entering into the structure of organised bodies, and to the creation of an unlimited number of carbon compounds, among which the colours derived from

coal tar stand prominently forward, surpass the synthetical powers of Nature itself. Yet this teaching, as applied to the structure of carbon compounds, is not on the face of it directly applicable to the investigation of other elements, because in examining the first it is possible to assume that the atoms of carbon have always a definite and equal number of affinities, while in the combinations of other elements this is evidently inadmissible. Thus, for example, an atom of carbon yields only one compound with four atoms of hydrogen and one with four atoms of chlorine in the molecule, while the atoms of chlorine and hydrogen unite only in the proportions of one to one. Simplicity is here evident and forms a point of departure from which it is easy to move forward with firm and secure tread. Other elements are of a different nature. Phosphorus unites with three and with five atoms of chlorine, and consequently the simplicity and sharpness of the application of structural conceptions are lost. Sulphur unites only with two atoms of hydrogen, but with oxygen it enters into higher orders of combination. The periodic relationship which exists among all the properties of the elements, such, for example, as their ability to enter into various combinations, and their atomic weights, indicate that this variation in atomicity is subject to one perfectly exact and general law, and it is only carbon and its near analogues which constitute cases of permanently preserved atomicity. It is impossible to recognise as constant and fundamental properties of atoms, powers which, in substance, have proved to be variable. But by abandoning the idea of permanence. and of the constant saturation of affinities—that is to say, by acknowledging the possibility of free affinities—many retain a comprehension of the atomicity of the elements "under given conditions"; and on this frail foundation they build up structures composed of chemical molecules, evidently only because the conception of manifold affinities gives, at once, a simple statical method of estimating the composition of the most complicated molecules.

I shall enter neither into details, nor into the various consequences following from these views, nor into the disputes which have sprung up respecting them (and relating especially to the number of isomers possible on the assumption of free affinities), because the foundation or origin of theories of this nature suffers from the radical defect of being in opposition to dynamics. The molecule, as even Laurent expressed himself, is represented as an architectural structure, the style of which is determined by the fundamental arrangement of a few atoms, while the decorative details, which are capable of being varied by the same forces, are formed by the elements entering into the combination. It is on this account that the term "structural" is so appropriate to the contemporary views of the above order, and that the "constructors" seek to justify the tetrahedric, plane, or prismatic disposition of the atoms of carbon in benzole. It is evident that the consideration relates to the statical position of atoms and molecules and not to their kinetic relations. The atoms of the

structural type are like the lifeless pieces on a chess board: they are endowed but with the voices of living beings, and are not those living beings themselves; acting, indeed, according to laws, yet each possessed of a store of energy, which, in the present state of our knowledge, must be taken into account.

In the days of Haüy, crystals were considered in the same statical and structural light, but modern crystallographers, having become more thoroughly acquainted with their physical properties and their actual formation, have abandoned the earlier views and have made their doctrines dependent on dynamics.

The immediate object of this lecture is to show that, starting with Newton's third law of motion, it is possible to preserve to chemistry all the advantages arising from structural teaching, without being obliged to build up molecules in solid and motionless figures, or to ascribe to atoms definite limited valencies, directions of cohesion, or affinities. The wide extent of the subject obliges me to treat only a small portion of it, namely of *substitutions*, without specially considering combinations and decompositions, and, even then, limiting myself to the simplest examples, which, however, will throw open prospects embracing all the natural complexity of chemical relations. For this reason, if it should prove possible to form groups similar, for example, to H^4 or CH^6 as the remnants of molecules CH^4 or C^2H^6 we shall not pause to consider them, because, as far as we know, they fall asunder into two parts, $H^2 + H^2$ or $CH^4 + H^2$, as soon as they are even temporarily formed, and are capable of separate existence, and therefore can take no part in the elementary act of substitution. With respect to the simplest molecules which we shall select—that is to say, those of which the parts have no separate existence, and therefore cannot appear in substitutions—we shall consider them according to the periodic law, arranging them in direct dependence on the atomic weight of the elements.

Thus, for example, the molecules of the simplest hydrogen compounds—

HF	H^2O	H^3N	H^4C
hydrofluoric acid	water	ammonia	methane

correspond to elements the atomic weights of which decrease consecutively,

$$F = 19, O = 16, N = 14, C = 12.$$

Neither the arithmetical order (1, 2, 3, 4 atoms of hydrogen) nor the total information we possess respecting the elements will permit us to interpolate into this typical series one more additional element; and therefore we have here, for hydrogen compounds, a natural base upon which are built up those simple chemical combinations which we take as typical. But even they are competent to unite with each other, as we see, for instance, in the property which hydrofluoric acid

has of forming a hydrate, that is of combining with water; and the similar attribute of ammonia, resulting in the formation of a caustic alkali, NH^3H^2O, or NH^4OH.

Having made these indispensable preliminary observations, I may now attack the problem itself and attempt to explain the so-called structure, or rather construction of molecules, that is to say, their constitution and transformations without having recourse to the teaching of "structionists," but on Newton's dynamical principles.

Of Newton's three laws of motion, only the third can be applied directly to chemical molecules when regarded as systems of atoms among which it must be supposed that there exist common influences or forces, and resulting compounded relative motions. Chemical reactions of every kind are undoubtedly accomplished by changes in these internal movements, respecting the nature of which nothing is known at present, but the existence of which, the mass of evidence collected in modern times, forces us to acknowledge as forming part of the common motion of the universe, and as a fact further established by the circumstance that chemical reactions are always characterised by changes of volume or the relations between the atoms or the molecules. Newton's third law, which is applicable to every system, declares that, "action is always associated with reaction, and is equal to it." The brevity and conciseness of this axiom was, however, qualified by Newton in a more expanded statement, "the action of bodies one upon another are always equal, and in opposite directions." This simple fact constitutes the point of departure for explaining dynamic equilibrium, that is to say, systems of conservancy. It is capable of satisfying even the dualists, and of explaining, without additional assumptions, the preservation of those chemical types which Dumas, Laurent, and Gerhardt created unit types, and those views of atomic combinations which the structionists express by atomicity or the valency of the elements, and, in connection with them, the various numbers of affinities. In reality if a system of atoms or a molecule be given, then in it, according to the third law of Newton, each portion of atoms acts on the remaining portion in the same manner, and with the same force as the second set of atoms acts on the first. We infer directly from this consideration that both sets of atoms, forming a molecule, are not only equivalent with regard to themselves, as they must be according to Dalton's law, but also that they may, if united, replace each other. Let there be a molecule containing atoms A B C, it is clear that, according to Newton's law, the action of A on B C must be equal to the action of B C on A, and if the first action is directed on B C, then the second must be directed on A, and consequently then, where A can exist in dynamic equilibrium, B C may take its place and act in a like manner. In the same way the action of C is equal to the action of A B. In one word every two sets of atoms forming a molecule are equivalent to each other, and may take each other's place in other molecules, or, having the power of balancing each other, the atoms or their comple-

ments are endowed with the power of replacing each other. Let us call this consequence of an evident axiom "the principle of substitution," and let us apply it to those typical forms of hydrogen compounds, which we have already discussed, and which, on account of their simplicity and regularity, have served as starting points of chemical argument long before the appearance of the doctrine of structure.

In the type of hydrofluoric acid, HF, or in systems of double stars, are included a multitude of the simplest molecules. It will be sufficient for our purpose to recall a few: for example the molecules of chlorine, Cl^2, and of hydrogen, H^2, and hydrochloric acid, HCL, which is familiar to all in aqueous solution as spirit of salt, and which has many points of resemblance with HF, HB_2, HI. In these cases division into two parts can only be made in one way, and therefore the principle of substitution renders it probable that exchanges between the chlorine and the hydrogen can take place, if they are competent to unite with each other. There was a time when no chemist would even admit the idea of any such action; it was then thought that the power of combination indicated a polar difference of the molecules in combination, and this thought set aside all idea of the substitution of one component element by another.

Thanks to the observations and experiments of Dumas and Laurent fifty years ago, such fallacies were dispelled, and in this manner, this same principle of substitution was exhibited. Chlorine and bromine acting on many hydrogen compounds, occupy immediately the place of their hydrogen, and the displaced hydrogen, with another atom of chlorine or bromine, forms hydrochloric acid or bromide of hydrogen. This takes place in all typical hydrogen compounds. Thus chlorine acts on this principle on gaseous hydrogen—reaction, under the influence of light, resulting in the formation of hydrochloric acid. Chlorine acting on the alkalis, constituted similarly to water, and even on water itself—only, however, under the influence of light and only partially because of the unstability of HClO—forms by this principle bleaching salts, which are the same as the alkalis, but with their hydrogen replaced by chlorine. In ammonia and in methane, chlorine can also replace the hydrogen. From ammonia is formed in this manner the so-called chloride of nitrogen, NCl^3, which decomposes very readily with violent explosion on account of the evolved gases, and falls asunder as chlorine and nitrogen. Out of marsh gas, or methane, CH^4, may be obtained consecutively, by this method, every possible substitution, of which chloroform, $CHCl^3$, is the best known, and chloro-carbonic acid, CCl^4 the most instructive. But by virtue of the fact that chlorine and bromine act, in the manner shown, on the simplest typical hydrogen compounds, their action on the more complicated ones may be assumed to be the same. This can be easily demonstrated. The hydrogen of benzole, C^6H^6, reacts feebly under the influence of light on liquid bromine, but Gustavson has shown that the addition of the smallest quantity of

metallic aluminium causes energetic action, and the evolution of large volumes of bromide of hydrogen,

If we pass on to the second typical hydrogen compound, that is to say water, its molecule, HOH, may be split up in two ways: either into an atom of hydrogen and a molecule of oxide of hydrogen, HO, or into oxygen, O, and two atoms of hydrogen, H; and therefore, according to the principle of substitution, it is evident that one atom of hydrogen can exchange with oxide of hydrogen, HO, and two atoms of hydrogen, H, with one atom of oxygen, O.

Both these forms of substitution will constitute methods of oxidation, that is to say, of the entrance of oxygen into the compound—a reaction which is so common in nature as well as in the arts, taking place at the expense of the oxygen of the air or by the aid of various oxidising substances or bodies which part easily with their oxygen. There is no occasion to reckon up the unlimited number of cases of such oxidising reactions. It is sufficient to state that in the first of these oxygen is directly transferred, and the position, the chemical function, which hydrogen originally occupied is, after the substitution, occupied by the hydroxyl. Thus ammonia, NH^3, yields hydroxylamine, $NH^2(OH)$, a substance which retains many of the properties of ammonia.

Methane and a number of other hydrocarbons yield, by substitution of the hydrogen by its oxide, methylic, $CH^3(OH)$, and other alcohols. The substitution of one atom of oxygen for two atoms of hydrogen is equally common with hydrogen compounds. By this means alcoholic liquids containing ethyl alcohol, or spirits of wine, $C^2H^5(OH)$, are oxidised till they become vinegar or acetic acid, $C^2H^3O(OH)$. In the same way caustic ammonia, or the combination of ammonia with water, NH^3H^2O, or $NH^4(OH)$, which contains a great deal of hydrogen, by oxidation exchanges four atoms of hydrogen for two atoms of oxygen, and become converted into nitric acid $NO^2(OH)$. This process of conversion of ammonia salts into saltpetre goes on in the fields every summer, and with especial rapidity in tropical countries. The method by which this is accomplished, though complex, though involving the agency of all-permeating micro-organisms, is, in substance, the same as that by which alcohol is converted into acetic acid, or glycol, $C^2H^4(OH)^2$, into oxalic acid, if we view the process of oxidation in the light of the Newtonian principles.

But while speaking of the application of the principle of substitution to water, we need not multiply instances, but must turn our attention to two special circumstances which are closely connected with the very mechanism of substitutions.

In the first place, the replacement of two atoms of hydrogen by one atom of oxygen may take place in two ways, because the hydrogen molecule is composed of two atoms, and therefore, under the influence of oxygen, the molecule forming water may separate before the oxygen has time to take its place. It is for this reason that we find, during

the conversion of alcohol into acetic acid, that there is an interval during which is formed aldehyde, C^2H^4O, which, as its very name implies, is "alcohol dehydrogenatum," or alcohol deprived of hydrogen. Hence aldehyde combined with hydrogen yields alcohol; and united to oxygen, acetic acid.

For the same reason there should be, and there actually are, intermediate products between ammonia and nitric acid, $NO^2(HO)$, containing either less hydrogen than ammonia, less oxygen than nitric acid, or less water than caustic ammonia. Accordingly we find, among the products of the de-oxidisation of nitric acid and the oxidisation of ammonia, not only hydroxylamine, but also nitrous oxide, nitrous and nitric anhydrides. Thus, the production of nitrous acid results from the removal of two atoms of hydrogen from caustic ammonia and the substitution of the oxygen for the hydrogen, NO (OH); or by the substitution, in ammonia, of three atoms of hydrogen by hydroxyl, $N(OH)^3$, and by the removal of water; $N(OH)^3 - H^2O = NO(OH)$. The peculiarities and properties of nitrous acid, as, for instance, its action on ammonia and its conversion, by oxidation, into nitric acid, are thus clearly revealed.

On the other hand, in speaking of the principle of substitution as applied to water, it is necessary to observe that hydrogen and hydroxyl, H and OH, are not only competent to unite, but also to form combinations with themselves, and thus become H^2 and H^2O^2; and such are hydrogen and the peroxide thereof. In general, if a molecule AB exists, then molecules AA and BB can exist also. A direct reaction of this kind does not, however, take place in water, therefore undoubtedly, at the moment of formation hydrogen reacts on the peroxide of hydrogen, as we can show, at once, by experiment; and further because the peroxide of hydrogen, H^2O^2, exhibits a structure containing a molecule of hydrogen, H^2, and one of oxygen, O^2, either of which is capable of separate existence. The fact, however, may now be taken as thoroughly established, that, at the moment of combustion of hydrogen or of the hydrogen compounds, peroxide of hydrogen is always formed, and not only so, but in all probability its formation invariably precedes the formation of water. This was to be expected as a consequence of the law of Avogadro and Gerhardt, which leads us to expect this sequence in the case of equal interactions of volumes of vapours and gases; and in the peroxide of hydrogen we actually have such equal volumes of the elementary gases.

The instability of peroxide of hydrogen—that is to say, the ease with which it decomposes into water and oxygen, even at the mere contact of porous bodies—accounts for the circumstance that it does not form a permanent product of combustion, and is not produced during the decomposition of water. I may mention this additional consideration that, with respect to the peroxide of hydrogen, we may look for its effecting still further substitutions of hydrogen by means of which we may expect to obtain still more highly oxidised water-compounds, such as H^2O^3 and H^2O^4. These Schönbein and

Bunsen have long been seeking, and Berthelot is investigating them at this moment. It is probable, however, that the reaction will stop at the last compound, because we find that, in a number of cases, the addition of 4 atoms of oxygen seems to form a limit. Thus, OsO^4, $KClO^4$, $KMnO^4$, K^2SO^4, Na^3PO^4, and such like, represent the highest grades of oxidation.*

As for the last 40 years, from the times of Berzelius, Dumas, Liebig, Gerhardt, Williamson, Frankland, Kolbe, Kekulé, and Butlerow, most theoretical generalisations have centred round organic or carbon compounds, so we will, for the sake of brevity, leave out the discussion of ammonia derivatives, notwithstanding their simplicity in respect to the doctrine of substitutions; we will dwell more especially on its application to carbon compounds, starting from methane, CH^4, as the simplest of the hydrocarbons, containing in its molecule one atom of carbon. According to the principles enumerated we may derive from CH^4 every combination of the form CH^3X, CH^2X^2, CHX^3, and CX^4, in which X is an element, or radical, equivalent to hydrogen, that is say, competent to take its place or to combine with it. Such are the chlorine substitutes mentioned already, such is wood-spirit, $CH^3(OH)$, in which X is represented by the residue of water, and such are numerous other carbon derivatives. If we continue, with the aid of hydroxyl, further substitutions of the hydrogen of methane we shall obtain successively $CH^2(OH)^2$, $CH(OH)^3$, and $C(OH)^4$. But if, in proceeding thus, we bear in mind that $CH^2(OH)^2$ contains two hydroxyls in the same form as peroxide of hydrogen, H^2O^2 or $(OH)^2$, contains them—and moreover not only in one molecule, but together, attached to one and the same atom of carbon—so here we must look for the same decomposition as that which we find in peroxide of hydrogen, and accompanied also by the formation of water as an independently existing molecule;

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* Because more than four atoms of hydrogen never unite with one atom of the elements, and because the hydrogen compounds (e. g. HCl, H^2S, H^3P, H^4Si) always form their highest oxides with four atoms of oxygen, and as the highest forms of oxides (OSO^4 RO^4) also contain four of oxygen, and eight groups of the periodic system, corresponding to the highest basic oxides R^2O, RO, R^2O^3, RO^2, R^2O^5, RO^3, R^2O^7, and RO^4, imply the above relationship, and because of the nearest analogues among the elements—such as Mg, Zn, Cd, and Hg; or Cr, Mo, W and U; or Si, Ge, Sn, and Pt; or F, Cl, Br, and J and so forth—not more than four are known, it seems to me that in these relationships there lies a deep interest and meaning with regard to chemical mechanics. But because, to my imagination, the idea of unity of design in Nature, either acting in complex celestial systems or among chemical molecules, is very attractive, especially because the atomic teaching at once acquires its true meaning, I will recall the following facts relating to the solar system. There are eight major planets, of which the four inner ones are not only separated from the four outer by asteroids, but differ from them in many respects, as for example in the smallness of their diameters and their greater density. Saturn with his ring has eight satellites, Jupiter and Uranus have each four. It is evident that in the solar systems also we meet with these higher numbers four and eight which appear in the combination of chemical molecules.

therefore $CH^2(OH)^2$ should yield, as it actually does, immediately water and the oxide of methylene, CH^2O, which is methane with oxygen substituted for two atoms of hydrogen. Exactly in the same manner out of $CH(OH)^3$ are formed water and formic acid, $CHO(OH)$, and out of $C(OH)^4$ is produced water and carbonic acid, or directly carbonic anhydride, CO^2, which will therefore be nothing else than methane with the double replacement of pairs of hydrogen by oxygen. As nothing leads to the supposition that the four atoms of hydrogen in methane differ one from the other, so it does not matter by what means we obtain any one of the combinations indicated—they will be identical; that is to say, there will be no case of actual isomerism, although there may easily be such cases of isomerism as have been distinguished by the term metamerism.

Formic acid, for example, has two atoms of hydrogen, one attached to the carbon left from the methane, and the other attached to the oxygen which has entered in the form of hydroxyl, and if one of them be replaced by some substance X it is evident that we shall obtain bodies of the same composition, but of different construction, or of different orders of movement among the molecules, and therefore endowed with other properties and reactions. If X be methyl, CH^3, that is to say, a group capable of replacing hydrogen because it is actually contained with hydrogen in methane itself, then by substituting this group for the original hydrogen, we obtain acetic acid, $CCH^3O(OH)$, out of formic, and by substitution of the hydrogen in its oxide or hydroxyl we obtain methyl formiate, $CHO(OCH^3)$. These bodies differ so much from each other physically and chemically that, at first sight, it is hardly possible to admit that they contain the same atoms in identically the same proportions. Acetic acid, for example, boils at a higher temperature than water, and has a higher specific gravity than it, while its metamer, formo-methylic ether, is lighter than water, and boils at 30°, that is to say, it evaporates very easily.

Let us now turn to carbon compounds containing two atoms of carbon to the molecule, as in acetic acid, and proceed to evolve them from methane by the principle of substitution. This principle declares at once that methane can only be split up in the four following ways:—

1. Into a group CH^3 equivalent with H. Let us call changes of this nature methylation.

2. Into a group CH^2 and H^2. We will call this order of substitutions methylenation.

3. Into CH and H^3, which commutations we will call acetylenation.

4. Into C and H^4, which may be called carbonisation.

It is evident that hydrocarbon compounds containing two atoms of carbon, can only proceed from methane, CH^4, which contains four atoms of hydrogen by the first three methods of substitution; carbonising would yield free carbon if it could take place directly, and if the

molecule of free carbon—which is in reality very complex, that is to say strongly polyatomic, as I have long since been proving by various means—could contain only C^2 like the molecules O^2, H^2, N^2, and so on.

By methylation we should evidently obtain from marsh gas, ethane, $CH^3CH^3 = C^2H^6$.

By methylenation, that is by substituting group CH^2 for H^2, methane forms ethylene, $CH^2CH^2 = C^2H^4$.

By acetylenation, that is by substituting three atoms of hydrogen, H^3, in methane, by the remnant CH, we get acetylene $CHCH = C^2H^2$.

If we have applied the principles of Newton correctly, there should not be any other hydrocarbons containing two atoms of carbon in the molecule. All these combinations have long been known, and in each of them we can not only produce those substitutions of which an example has been given in the case of methane, but also all the phases of other substitutions, as we shall find from a few more instances, by the aid of which I trust that I shall be able to show the great complexity of those derivatives which, on the principle of substitution, can be obtained from each hydrocarbon. Let us content ourselves with the case of ethane, CH^3CH^3, and the substitution of the hydrogen by hydroxyl. The following are the possible changes.

1. $CH^3CH^2(OH)$: this is nothing more than spirit of wine, or ethyl alcohol, $C^2H^5(OH)$ or C^2H^6O.

2. $CH^2(OH)CH^2(OH)$: this is the glycol of Wurtz, which has shed so much light on the history of alcohol. Its isomer may be $CH^3CH(OH)^2$, but as we have seen in the case of $CH(OH)^2$, it decomposes giving off water, and forming aldehyde, CH^3CHO, a body capable of yielding alcohol by uniting with hydrogen and of yielding acetic acid by uniting with oxygen.

If glycol $CH^2(OH)CH^2(OH)$ loses its water, it may be seen at once that it will not now yield aldehyde, CH^3CHO, but its isomer, $\underset{O}{CH^2CH^2}$, the oxide of ethylene. I have here indicated in a special manner the oxygen which has taken the place of two atoms of the hydrogen of ethane taken from different atoms of the carbon.

3. $CH^3C(OH)^3$ decomposed as $CH(OH)^3$, forming water and acetic acid $OH^3CO(OH)$. It is evident that this acid is nothing else than formic acid, $CHO(OH)$, with its hydrogen replaced by methyl. Without examining further the vast number of possible derivatives, I will direct your attention to the circumstance that in dissolving acetic acid in water we obtain the maximum contraction and the greatest viscosity when to the molecule $CH^3CO(OH)$ is added a molecule of water, which is the proportion which would form the hydrate $CH^3C(OH)^3$. It is probable that the doubling of the molecule of acetic acid at temperatures approaching its boiling point has some connection with this power of uniting with one molecule of water.

4. $CH^2(OH)C(OH)^3$ is evidently alcoholic acid, and indeed this compound, after losing water, answers to glycolic acid, $CH^2(OH)CO$

(OH). Without investigating all the possible isomers, we will note only that the hydrate $CH(OH)^2CH(OH)^2$ has the same composition as $CH^2(OH)C(OH)^3$, and although corresponding to glycol, and being a symmetrical substance, it becomes on parting with its water aldehyde of oxalic acid, or the glyoxal of Debus, CHOCHO.

5. $CH(OH)^2C(OH)^3$, from the tendency of all the preceding, corresponds to glyoxylic acid, aldehyde acid, CHOCO(OH), because the group CO(OH), or carboxyl, enters into the compositions of organic acids, and the group CHO defines the aldehyde function.

6. $C(OH)^3C(OH)^3$ through the loss of $2H^2O$ yields the bibasic oxalic acid CO(OH)CO(OH), which generally crystallises with $2H^2O$, following thus the normal type of hydration characteristic of ethane.*

Thus, by applying the principle of substitution, we can, in the simplest manner, derive not only every kind of hydrocarbon compound, such as the alcohols, the aldehyde alcohols, aldehydes, alcohol acids, and the acids, but also combinations analogous to hydrated crystals which usually are disregarded.

But even those unsaturated substances, of which ethylene, CH^2CH^2, and acetylene, CHCH, are types, may be evolved with equal simplicity. With respect to the phenomena of isomerism, there are many possibilities among the hydrocarbon compounds containing two atoms of carbon, and without going into details it will be sufficient to indicate that the following formulæ, though not identical, will be isomeric substantially among themselves:—CH^3CHX^2 and CH^2XCH^2X, although both contain $C^2H^4X^2$, or CH^2CX^2 and CHXCHX, although both contain $C^2H^2X^2$, if by X we indicate chlorine or generally an element capable of replacing one atom of hydrogen, or capable of uniting with it. To isomerism of this kind belongs the case of aldehyde and the oxide of ethylene, to which we have already referred, because both have the composition C^2H^4O.

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* One more isomer, $CH^2CH(OH)$, is possible, that is secondary vinyl alcohol, which is related to ethylene, CH^2CH^2, but derived by the principle of substitution from CH^4. Other isomers of the composition C^2H^4O, such, for example, as $CHCH^2(OH)$, are impossible, because it would correspond to the hydrocarbon $CHCH^3 = C^2H^4$, which is isomeric with ethylene, and it cannot be derived from methane. If such an isomer existed, it would be derived from CH^2, but such products are up to the present unknown. In such cases the insufficiency of the points of departure of the statical structural teaching is shown. It first admits constant atomicity and then rejects it, the facts serving to establish either one or the other view; and therefore, it seems to me, that we must come to the conclusion that the structural method of reasoning, having done a service to science, has outlived the age, and must be regenerated as, in their time, was the teaching of the electro-chemists, the radicalists, and the adherents of the doctrine of types. As we cannot now lean on the views above stated, it is time to abandon the structural theory. They will all be united in chemical mechanics, and the principle of substitution must be looked upon only as a preparation for the coming epoch in chemistry, where such cases as the isomerism of fumaric and maleic acids, when explained dynamically, as proposed by Le Bel and Van't Hoff, may yield points of departure.

What I have said appears to me sufficient to show that the principle of substitution adequately explains the composition, the isomerism and all the diversity of combination of the hydrocarbons, and I shall limit the further development of these views to preparing a complete list of every possible hydrocarbon compound containing three atoms of carbon in the molecule. There are eight in all, of which only five are known at present.*

Among those possible for C^3H^6 there should be two isomers, propylene and trimethylene, and they are both already known. For C^3H^4 there should be three isomers: allylene and allene are known, but the third has not yet been discovered; and for C^3H^2 there should be two isomers, though neither of them are known as yet. Their composition and structure is easily deduced from ethane, ethylene, and acetylene, by methylation, methylenation, by acetylenation and by carbonisation.

1. $C^3H^8 = CH^3CH^2CH^3$ out of CH^3CH^3 by methylation. This hydrocarbon is named propane.

2. $C^3H^6 = CH^3CHCH^2$ out of CH^3CH^3 by methylenation. This substance is propylene.

3. $C^3H^6 = CH^2CH^2CH^2$ out of CH^3CH^3 by methylenation. This substance is trimethylene.

4. $C^3H^4 = CH^3CCH$ out of CH^3CH^3 by acetylenation or from $CHCH$ by methylation. This hydrocarbon is named allylene.

5. $C^3H^4 = \begin{array}{c}CHCH\\ CH^2\end{array}$ out of CH^3CH^3 by acetylenation, or from CH^2CH^2 by methylenation, because $\begin{array}{c}CH^2CH\\ CH\end{array} = \begin{array}{c}CHCH.\\ CH^2\end{array}$ This body is as yet unknown.

6. $C^3H^4 = CH^2CCH^2$ out of CH^2CH^2 by methylenation. This hydrocarbon is named allene, or iso-allylene.

7. $C^3H^2 = \begin{array}{c}CHCH\\ C\end{array}$ out of CH^3H^3 by symmetrical carbonisation, or out of CH^2CH^2 by acetylenation. This compound is unknown.

8. $C^3H^2 = \begin{array}{c}CC\\ CH^2\end{array}$ out of CH^3CH^3 by carbonisation, or out of $CHCH$ by methylenation. This compound is unknown.

If we bear in mind that for each hydrocarbon serving as a type in the above tables there are a number of corresponding derivatives, and that every compound obtained may, by further methylation, methylenation, acetylenation, and carbonisation, produce new hydrocarbons, and these may be followed by a numerous suite of derivatives and an immense number of isomeric bodies, it is possible to understand the limitless number of carbon compounds, although they all

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* Conceding variable atomicity, the structurists must expect an incomparably larger number of isomers, and they cannot now decline to acknowledge the change of atomicity, were it only for the examples $HgCl$ and $HgCl^2$, CO and CO^2, PCl^3 and PCl^5.

have the one substance, methane, for their origin. The number of substances is so enormous that it is no longer a question of enlarging the possibilities of discovery, but rather of finding some means of testing them, analogous to the well-known two which for a long time have served as gauges for all carbon compounds.

I refer to the law of even numbers and to that of limits, the first enunciated by Gerhardt forty years ago, with respect to hydrocarbons, namely, that their molecules always contain an even number of atoms of hydrogen. But by the method which I have used of deriving all the hydrocarbons from methane, CH^4, this law may be deduced as a direct consequence of the principle of substitutions. Accordingly, in methylation, CH^3 takes the place of H, and therefore CH^2 is added. In methylenation the number of atoms of hydrogen remains unchanged, and at each acetylenation it is reduced by two, and in carbonisation by four atoms, that is to say, an even number of atoms of hydrogen is always added or removed. And because the fundamental hydrocarbon, methane, CH^4, contains an even number of atoms of hydrogen, therefore all its derivative hydrocarbons will also contain even numbers of hydrogen, and this constitutes the law of even numbered parts.

The principle of substitutions explains with equal simplicity the conception of limiting compositions of hydrocarbons, C^nH^{2n+2}, which I derived, in 1861,* in an empirical manner from accumulated materials available at that time, and on the basis of the limits to combinations worked out by Dr. Frankland for other elements.

Of all the various substitutions the highest proportion of hydrogen is yielded by methylation, because in that operation alone does the quantity of hydrogen increase; therefore, taking methane as a point of departure, if we imagine methylation effected $(n-1)$ times we obtain hydrocarbon compounds containing the highest quantities of hydrogen. It is evident that they will contain

$$CH^4 + (n-1)\,CH^2, \text{ or } C^nH^{2n+2},$$

because methylation leads to the addition of CH^2 to the compound.

It will thus be seen that by the principle of substitution—that is to say, by the third law of Newton—we are able to deduce, in the simplest manner, not only the individual composition, the isomerism, and relations of substances, but also the general laws which govern their most complex combinations, without having recourse either to statical constructions, to the definition of atomicities, to the exclusion of free affinities, or to the recognition of those single, double, or treble ties which are so indispensable to structurists in the explanation of the composition and construction of hydrocarbon compounds. And yet, by the application of the dynamic principles of Newton, we can

* 'Essai d'une théorie sur les limites des combinaisons organiques,' par D. Mendeléeff, 2/11 août 1861, 'Bulletin de l'Académie i. d. Sc. de St. Pétersbourg,' t. v.

attain to that chief and fundamental object—the comprehension of isomerism in hydrocarbon compounds, and the forecasting of the existence of combinations as yet unknown, by which the ediñce raised by structural teaching is strengthened and supported. Besides, and I count this for a circumstance of special importance, the process which I advocate will make no difference in those special cases which have been already so well worked out, such as, for example, the isomerism of the hydrocarbons and alcohols, even to the extent of not interfering with the nomenclature which has been adopted, and the structural system will retain all the glory of having worked up, in a thoroughly scientific manner, the store of information which Gerhardt had accumulated about the middle of the fifties, and the still higher glory of establishing the rational synthesis of organic substances. Nothing will be lost to the structural doctrine, except its statical origin; and as soon as it will embrace the dynamic principles of Newton, and suffer itself to be guided by them, I believe that we shall attain, for chemistry, that unity of principle which is now wanting. Many an adept will be attracted to that brilliant and fascinating enterprise, the penetration into the unseen world of the kinetic relations of atoms, to the study of which the last twenty-five years have contributed so much labour and such high inventive faculties.

D'Alembert found in mechanics, that if inertia be taken to represent force, dynamic equations may be applied to statical questions which are thereby rendered more simple and more easily understood.

The structural doctrine in chemistry has unconsciously followed the same course, and therefore its terms are easily adopted; they may retain their present forms provided that a truly dynamical, that is to say, Newtonian meaning be ascribed to them.

Before finishing my task and demonstrating the possibility of adapting structural doctrines to the dynamics of Newton, I consider it indispensable to touch on one question which naturally arises, and which I have heard discussed more than once. If bromine, the atom of which is eighty times heavier than that of hydrogen, takes the place of hydrogen, it would seem that the whole system of dynamic equilibrium must be destroyed.

Without entering into the minute analysis of this question, I think it will be sufficient to examine it by the light of two well-known phenomena, one of which will be found in the department of chemistry, and the other in that of celestial mechanics, and both will serve to demonstrate the existence of that unity in the plan of creation, which is a consequence of the Newtonian doctrines. Experiments demonstrate that when a heavy element is substituted for a light one, in a chemical compound—an atom of magnesium in the oxide of that metal, for example, for mercury, the atom of which is $8\frac{1}{3}$ times heavier—the chief chemical characteristics or properties are generally though not always preserved.

The substitution of silver for hydrogen, than which it is 108 times heavier, does not affect all the properties of the substance,

though it does some. Therefore chemical substitutions of this kind, the substitution of light for heavy atoms, need not necessarily entail changes in the original equilibrium; and this point is still further elucidated by the consideration that the periodic law indicates the degree of influence of an increment of weight in the atom as affecting the possible equilibria, and also what degree of increase in the weight of the atoms reproduces some, though not all, the properties of the substance.

This tendency to repetition, these periods, may be likened to those annual or diurnal periods with which we are so familiar on the earth. Days and years follow each other: but, as they do so, many things change; and in like manner chemical evolutions, changes in the masses of the elements, permit of much remaining undisturbed, though many properties undergo alteration. The system is maintained according to the laws of conservation in nature, but the motions are altered in consequence of the change of parts.

Next, let us take an astronomical case, such for example as the earth and the moon, and let us imagine that the mass of the latter is constantly increasing. The question is, what will then occur? The path of the moon in space is a wave-line similar to that which geometricians have named epicycloidal, or the locus of a point in a circle rolling round another circle. But in consequence of the influence of the moon, it is evident that the path of the earth itself cannot be a geometric ellipse, even supposing the sun to be immovably fixed; it must be an epicycloidal curve, though not very far removed from the true ellipse, that is to say, it will be impressed with but faint undulations. It is only the common centre of gravity of the earth and the moon which describes a true ellipse round the sun. If the moon were to increase, the relative undulations of the earth's path would increase in amplitude, those of the moon would also change, and when the mass of the moon had increased to an equality with that of the earth, the path would consist of epicycloidal curves crossing each other, and having opposite phases. But a similar relation exists between the sun and the earth because the former is also moving in space. We may apply these views to the world of atoms, and suppose that, in their movements, when heavy ones take the place of those that are lighter, similar changes take place provided that the system or the molecule is preserved throughout the change.

It seems probable that in the heavenly systems, during incalculable astronomical periods changes have taken place and are still going on similar to those which pass rapidly before our eyes during the chemical reaction of molecules and the progress of molecular mechanics, may—we hope will—in course of time, permit us to explain those changes in the stellar world which have more than once been noticed by astronomers, and which are now so carefully studied. A coming Newton will discover the laws of these changes. Those laws, when applied to chemistry, may exhibit peculiarities, but these

will certainly be mere variations on the grand harmonious theme which reigns in nature. The discovery of the laws which produce this harmony in chemical evolutions will only be possible, it seems to me, under the banner of Newtonian dynamics which have so long waved over the domains of mechanics, astronomy, and physics. In calling chemists to take their stand under its peaceful and catholic shadow I imagine that I am aiding in establishing that scientific union which the managers of the Royal Institution wish to effect, who have shown their desire to do so by the flattering invitation which has given me—a Russian—the opportunity of laying before the countrymen of Newton an attempt to apply to chemistry one of his immortal principles.

[D. M.]

GENERAL MONTHLY MEETING,

Monday, June 3, 1889.

Sir James Crichton Browne, M.D. LL.D. F.R.S. Vice-President, in the Chair.

Miss Beatrice Harvey,
Reginald Ward, Esq.

were elected Members of the Royal Institution.

The Honorary Secretary reported the decease of Mr. Henry Pollock, Treasurer and Vice-President, on the 15th of May last.

The following Resolution passed by the Managers at their Meeting this day was read:—

Resolved, "It is with the deepest regret that the Managers have to record the loss of their Treasurer and Vice-President, Mr. Henry Pollock.

"Elected a Member of the Royal Institution thirty-five years ago, he has ever since shown the warmest interest in the prosperity of the Institution, and has devoted himself to its advancement in every way. Several years he was a Manager, and for the last three years he has been our Treasurer, and in that capacity, has most materially benefited the Institution by his business aptitude and by his close attention to its affairs.

"Although during a period of some two years the state of his health has not permitted him to be present at our Meetings, he has nevertheless, for the greater part of that time, still attended to our interests, by going over the accounts at his own house, and by seeing, from time to time, the Secretary and Professor Dewar, when occasion has arisen for consultation upon any important matter.

"The Managers feel that, in the loss of a colleague so very earnest for the welfare of the Institution; so thoroughly able in the discharge of his duties; and

so kindly and courteous to all, the Institution has been deprived of one of its most proved and useful Members."

Resolved, "That the Honorary Secretary do transmit to Mrs. Henry Pollock a copy of the foregoing Resolution, together with an expression of the deep sympathy entertained by the Managers with her in her great bereavement."

The Special Thanks of the Members were returned for the following Donations to the Fund for the Promotion of Experimental Research:—

Mrs. J. Gibbs	£10 10
Ludwig Mond, Esq.	100 0

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz:—

FROM

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Academy of Natural Sciences, Philadelphia—Proceedings, 1888, Part 2. 8vo.
Agricultural Society of England, Royal—Journal, Vol. XXV. Part 1. 8vo. 1889.
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Catalogue of Chemical Periodicals. 8vo. 1885.
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Chemist and Druggist for May, 1889. 8vo.
Electrical Engineer for May, 1889. fol.
Engineer for May, 1889. fol.
Engineering for May, 1889. fol.
Horological Journal for May, 1889. 8vo.
Industries for May, 1889. fol.
Iron for May, 1889. 4to.
Murray's Magazine for May, 1889. 8vo.
Nature for May, 1889. 4to.
Photographic News for May, 1889. 8vo.
Revue Scientifique for May, 1889. 4to.
Telegraphic Journal for May, 1889. 8vo.
Zoophilist for May, 1889. 4to.
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Madrid Royal Academy of Sciences—Anuario, 1889. 8vo.
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Weekly Weather Reports, Nos. 1-21. 4to. 1889.
Middlesex Hospital—Reports, 1887. 8vo. 1888.
National Life-Boat Institution, Royal—Annual Report, 1889. 8vo.
Odontological Society of Great Britain—Transactions, Vol. XXI. Nos. 6, 7. New Series. 8vo. 1888.
Pharmaceutical Society of Great Britain—Journal, May, 1889. 8vo.
Photographic Society—Journal, Vol. XIII. No. 5. 8vo. 1889.
Richards, Admiral Sir G. H. K.C.B. F.R.S. &c. (the Conservator)—Report on the Navigation of the River Mersey, 1888. 8vo. 1889.
Rio de Janeiro Observatory—Revista, No. 3. 8vo. 1889.
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Bulletin, Tome XXXIII. No. 1. 4to. 1889.
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University of London—Calendar, 1889–90. 8vo.
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Vernon-Harcourt, L. F. Esq. M.A. (the Author)—The Principles of training Rivers through Tidal Estuaries (Proc. Roy. Soc. 45). 8vo. 1889.
Victoria Institute—Transactions, No. 88. 8vo. 1889.
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WEEKLY EVENING MEETING,

Friday, June 7, 1889.

Colonel James A. Grant, C.B. C.S.I. F.R.S. Vice-President, in the Chair.

Archibald Geikie, Esq. LL.D. F.R.S.
DIRECTOR-GENERAL OF THE GEOLOGICAL SURVEY OF THE UNITED KINGDOM.

Recent Researches into the Origin and Age of the Highlands of Scotland and the West of Ireland.

The records of geological history, like those of the human race, become more fragmentary and illegible, the farther back we trace them into the past. While the younger rocks of the earth's crust have been made to yield a more or less connected story of geographical and biological evolution, the oldest rocks have till comparatively lately been neglected, or have been tacitly left to mere speculation and conjecture. Only within the last few years have these ancient formations been seriously and sedulously attacked by scientific methods of inquiry. Though the progress of investigation has necessarily been slow, a steady advance in knowledge can be chronicled. There is a curious fascination in this department of geology. These venerable rocks reveal to us the oldest known part of the outer shell of our planet. The palimpsest of the earth's surface has been written over again and again during the long ages of geological history ; but down among these bottom-rocks we reach the earliest recognisable inscriptions, and come as near towards the beginning of things as geological evidence by itself is ever likely to lead us. These records carry us back to a time anterior to that of the oldest fossiliferous formations, possibly to an epoch that preceded the appearance of vegetable or animal life on the globe. They reveal to us the very foundations of the earth's crust, on which all other known rocks rest, and out of the waste of which the greater part of these rocks has been formed.

Within the last ten years, after prolonged misconception and neglect, the most ancient rocks of the British Isles have come to occupy a foremost place among the researches of the geologists of this country. The tracts where they are now exposed to view, often among the wildest mountains, or "placed far amid the melancholy main," have become favourite geological hunting grounds, and have furnished a notable amount of material for those disputes and combats which seem to form a necessary element in geological progress. Avoiding, as far as possible, matters of controversy, I propose this evening to offer a brief outline of the actual state of knowledge, up to the present time, of the history of those ancient crystalline masses

of which our north-western mountains are composed.* The story is a somewhat involved and complicated one. But its main points may perhaps be conveniently grasped, if we bear in mind that they naturally group themselves into four sections; 1st, the Archæan period; 2nd, the Cambrian period; 3rd, the Lower Silurian period; 4th, the period of the younger Schists.

Let me at the outset remark that in the investigation of these early ages of geological history we enjoy in this country a special advantage. The British Isles stand on the oceanic border of a great continental region. They are therefore placed along that critical belt where not only have terrestrial disturbances been especially numerous and violent from the earliest geological times, but where an oscillation upward or downward of a few hundred feet has sufficed to make all the difference between land and sea. In the heart of a continent, as, for example, over the vast plains of Russia, long cycles of geological time have passed without serious disturbance of any kind. To this day some of the ancient Palæozoic sediments in that region, for hundreds of square miles in extent, lie as level as when they were deposited on the sea-floor. They have been uplifted bodily into land, but still remain little more than mere hardened mud and sand. In Western Europe, on the other hand, where from the remotest geological antiquity the oscillations and dislocations have been innumerable, every successive continental uplift has recorded itself in some crumpling or fracture of the rocks. Hence in the geological map of that region the various formations form a pattern of exceeding complexity, while in the maps of Eastern Europe each of them covers a broad unbroken expanse.

I. The Archæan Period.

The oldest known rocks of Europe, now generally termed Archæan, are well exposed along the north-western borders of the continental area from the extreme north of Scandinavia, by the west coast of Scotland, to Galway Bay in the west of Ireland, a total distance of some 1600 miles. They give rise to topographical features which, where fully developed, strongly distinguish them from all younger formations. Nowhere else can such extraordinary unevenness of surface be found. Knobs, hummocks, and ridges of bare or almost bare rock, separated by narrow gullies or by wider winding valleys, roughen the ground in every direction. In the hollows lie innumerable tarns and lakes, or flat tracts of bog where lakes once were. In some districts, indeed, there is as much water as land in a given number of square miles. On a large scale, this type of scenery is perhaps best displayed in Finland; on a small scale, it is repeated

* It would be obviously out of place to include here references to the voluminous literature of the subject. A condensed summary will be found in the Report by the officers of the Geological Survey, 'Quart. Journ. Geol. Soc.' vol. xliv. 1888.

all through the chain of the outer Hebrides, as well as on the Archæan areas of the mainland. The most southerly points in Scotland where it can be recognised are the island of Iona and the Ross of Mull. It reappears, however, far to the south in Ireland; standing out in the bold cliffs from Erris Head to Achill Island in the west of Mayo, and finally covering an area of more than 500 square miles in south-western Galway. In this last named district, as Professor Hull has shown, so completely are the scenic features of the north-west of Scotland reproduced, down even to the minutest details, that the geologist, even before he stands on the rocks, has no difficulty in deciding that they can only be Archæan.

What, then, are these most ancient rocks of north-western Europe, and what has been their history? Unfortunately the answer to these questions cannot be succinctly and definitely given. Owing to the antiquity of the masses, and the prolonged series of geological revolutions which they have undergone, their original characters have been somewhat effaced. In those areas where they have been least altered, and where, therefore, they approach nearest to their primitive structure, they have been found by my colleagues of the Geological Survey to be crystalline rocks, such as gabbros, diorites, and other highly basic compounds. These occur in zones or bosses surrounded by and passing into rocks which have acquired the peculiarly banded structure characteristic of gneiss. That these various rocks were eruptive, that is, that they originally formed portions of igneous material that rose in a molten or plastic condition from below, can hardly be doubted. They remind us of the deep-seated portions of some of the eruptive bosses so abundantly intruded into the crust of the earth, and now so plentifully exposed at the surface after prolonged denudation. Like these, they show a rudely striped or banded arrangement suggestive of the planes of movement or flow-structure seen in consolidated igneous material. They have probably resulted from successive protrusions of eruptive rocks at some depth within the crust of the earth.

Nowhere, however, in the region to which I am referring has any trace of superficial eruption yet been detected. There are no true volcanic ejections, nor any evidence that the rocks, though certainly of eruptive origin, were ever connected with the ordinary explosive operations of volcanic vents. Not only so, but after the most careful search from Sutherland to Galway not a vestige have we yet found of any unquestionable sedimentary material. There are no conglomerates, no sandstones, no shales; nor even any materials that might be supposed to represent these in a metamorphosed condition. Of the actual surface of the earth these Archæan rocks afford no recognisable trace. They obviously did not form the superficial layer themselves. They must have lain deep under a cover of other material, under which they acquired their crystalline structure, and by the subsequent removal of which they have been exposed to the light.

One of the most impressive features of our recent researches among these rocks is the evidence of the magnitude of the interval of time between their original protrusion and the formation of the next group of rocks overlying them. Of the many breaks in the geological record, none is more complete than this. We pass at one step from Archæan rocks, dating no doubt from an early stage in the consolidation of the crust of the planet, to the gravelly and sandy deposits of an inland sea, which already present all the familiar characters of the sedimentary accumulations of later geological time.

Some of the more prominent events in this protracted interval may be more or less clearly discerned; others can only be dimly conjectured. Arranging in chronological order the more important which have lately been recognised by the Geological Survey, I would direct your attention to four main episodes in the Archæan history of our north-western Highlands.*

In the first place, the crust of the earth over that region was thrown into a series of low arches or folds, the axes of which ran in a general north-east and south-west direction. Its component rocks were crushed and sheared, so as to acquire the banded and crumpled structure of typical gneiss. Perhaps we may trace to these primeval terrestrial movements the first shaping of the European continent, which certainly has grown from north to south. At all events, it is interesting to note that the undulations into which the rocks were thrown took that north-easterly trend which is still so marked in the long belt of crystalline schists from the North Cape all the way to the west of Ireland.

In the second place after these early disturbances, and probably long after them, a remarkable series of manifestations of plutonic energy occurred. The region extending from the north-west of Scotland to the west of Ireland was convulsed by the production of innumerable dislocations in the solid terrestrial crust, having a general west-north-west direction. Up these gaping rents molten basic lava rose from some subterranean reservoir, and solidified in broad dykes of black basalt. Some of these dykes can be traced for ten or twelve miles till they run out to sea at the one end and pass under younger overlying formations at the other. Yet again at a somewhat later period another series of fissures was opened slightly oblique to the direction of the first, and in these still more basic lava formed a second series of dykes trending nearly east and west. Nor was this all, for there followed a third period of convulsion which gave birth to a series of huge dykes of granite.

Whether or not any of the eruptive material that filled these successive fissures ever rose to the surface and flowed out there, or gave rise to the explosive phenomena of true volcanic vents cannot be certainly affirmed. But an interesting piece of evidence points to the

* Those who wish fuller details on this subject will find them in the Survey Report already quoted.

probability that such a connection with the surface was really established. In some of the conglomerates of the next succeeding group (Cambrian or Torridon sandstones), there occur fragments of highly vesicular lavas, which show that at some time previous to the deposit of these coarse sediments, active volcanic vents existed somewhere in the region of the north-west of Scotland. As yet, however, no trace has been discovered of any of the lava-streams which flowed out at the surface.

Although volcanic energy has long been quiescent over the British Isles, probably no area in Europe exhibits within so limited a space so long and varied a record of volcanic eruptions. There is, therefore, a peculiar interest about these traces of the ancient volcanoes which in Archæan time rose along the Atlantic border in the north-west of Scotland, for they stand at the very beginning of that long history. Moreover, so far as we can interpret their remains, they seem in a curious way to have anticipated the characteristics of the last great volcanic episode in Britain—that to which we owe the Tertiary basaltic plateaux of Antrim and the Inner Hebrides. In both cases, the distinguishing feature was the fissuring of the terrestrial crust and the uprise of basic lava in the rents, with the consequent production of innumerable parallel dykes trending in a general north-westerly direction.

In the third place, after the production of the basic dykes, there came another prolonged interval, during which a series of remarkable terrestrial disturbances affected the north-west of Scotland. The crust of the earth in that part of Europe was once more dislocated by innumerable fissures, produced probably at successive epochs of paroxysm, for they can be grouped into three distinct series. Of these, one runs approximately parallel with the north-west dykes, the second trends east and west, and the third runs north-east and south-west, or north and south. So far as yet discovered, no lava of any kind welled upwards into these fissures. They are ruptures, but not dykes. They were accompanied, however, by the manifestation of another form of terrestrial energy, the geological efficacy of which has only recently been recognised. The lines of vertical fracture became also lines of horizontal or oblique movement during the vast strain of terrestrial contraction. One side was driven past the other side, and with such irresistible force that the rocks for some distance on either side were dragged into the line of movement, crushed down, and forced to assume a new crystalline arrangement of their materials. The basalt dykes, reduced sometimes from a width of 50 or 60 yards to only four feet or less, were changed into diorites, and where the shearing was greatest, into hornblende-schists. The gneiss, in like manner, was thrown into sharp folds, and had a newer foliation developed in it parallel with the new planes of movement.

In the fourth place, during the prolonged succession of changes which I have thus briefly summarised, there must have been in progress a continuous denudation of the surface of the Archæan land in

the north-west of Europe. Doubtless, each of the subterranean disturbances more or less affected the surface. The land was by degrees ridged up above the sea, and its height and breadth were probably from time to time increased by local uplifts accompanying the disturbances. But as soon as the land appeared, it began to be attacked by the waves, the air, rain, and running water. Terrestrial convulsions were intermittent, but superficial waste continued uninterrupted. Whatever may have been the character of its topography, the first formed land, as soon as it rose, became a prey to the denuding forces, and had its original surface gradually stripped off. We have no means of telling how great a thickness of material was in this manner removed from the land before the time of the next geological period, nor for how vast a time this slow process of denudation went on. All that we can now discover is a series of detached fragments of the surface of this primeval Europe, which have been preserved by being buried under the pile of material formed out of the waste of the Archæan rocks. From these fragments we learn that the rocks had been enormously denuded so as to lay bare to the surface some of their deep-seated parts, the land shaped out of them having been carved into dome-shaped hills and basin-like hollows, not very different from those which are so characteristic of the Archæan tracts to-day.

II. The Cambrian Period.

We now reach the base of the stratified formations of the British Isles, and enter upon a series of records which deal not with subterranean but with superficial changes, and in which the earliest geographical conditions of our area are more or less fully chronicled. These records consist of a pile of dull-red sandstones, conglomerates, and breccias, with grey, green, and black mudstones, marls and shales, attaining a maximum thickness of perhaps 10,000 feet. This great accumulation, chiefly of coarse sediment, was derived from the waste of the Archæan land. The pebbles in its conglomerates are fragments of that land, and enable us to form some conjecture as to the nature of the materials that composed its surface. An examination of these pebbles brings to light the important fact that besides the detritus of the gneiss and other Archæan rocks which can now be seen *in situ*, the conglomerates are made up of materials derived from some still older sedimentary formations which have entirely disappeared from our area. These included such rocks as quartzite, greywacke, shale, and limestone, besides abundant pieces from the lavas, which I have already referred to as having probably been erupted to the surface in pre-Cambrian time. The destruction of these intervening deposits, and the chance discovery that they once existed because fragments of them have been found in later conglomerates, serve to impress upon us the imperfection of the Geological Record and the vastness of the intervals of time which may sometimes separate two successive groups of rock.

The thick mass of red sandstone and conglomerate, which rests directly on the Archæan gneiss, forms some of the most singular scenery in the north-west of Scotland. Owing to vast denudation, which began before the next group of strata was deposited, it has been worn down into isolated mountains, which rise like a chain of colossal pyramids along the western shores of Sutherland and Ross. The almost level lines of stratification give to these eminences a look of architectural symmetry, in striking contrast with the more tumultuous aspect of the other rocks of the region, while their red tone of colour marks them out boldly from the wastes of grey gneiss below and the crags of white quartzite beyond. From the far northern cliffs of Sutherland these massive red sandstones can be followed almost continuously to the southern headlands of Skye. They reappear in great force in the island of Rum, beyond which they are not certainly traceable. A group of highly altered grits and schists, seen under the great basaltic plateau of Gribun, on the west side of the island of Mull, may mark their extreme southerly limits.* The red sandstones certainly do not come so far south as Iona, and not a trace of them has been met with in Ireland. They extend westwards across the Minch, for a small portion of them skirts the eastern shore of the Long Island. How far they may have stretched eastward cannot now be determined, for their limits in that direction have been obscured or effaced by the extraordinary series of gigantic earth-movements to be afterwards referred to. There can be little doubt, however, that they did not reach the district east of the line of the Great Glen, though they not improbably lay in thick mass over much of the country to the west of that valley.

We cannot now trace the original limits of these red rocks, yet we can hardly doubt that they never covered an area at all comparable in extent to that of the rocks below and above them. They appear, indeed, to have been accumulated in one or more basins, shut off from free communication with the open sea, where the deposition of ferruginous precipitates among the ordinary mechanical sediment could go on during the deposition of many thousand feet of rock. Such conditions of sedimentation were not very favourable to the existence of life in the waters of these enclosed basins. Nevertheless, that the waters were not entirely lifeless is shown by the discovery of organic remains on two widely separated horizons among the sandstones. These remains occur in grey and dark shales, the colour and composition of which suggest a temporary influx of water from without and the cessation for a time of the deposition of the iron-oxide. At the lower horizon the fossils consist of calcareous rods, the organic grade of which is still in dispute; at the higher they include some

* My attention was called to these rocks by the Duke of Argyll, who himself suggested their possible Cambrian age. I visited them this spring, and found them to be greatly metamorphosed. They do not appear in Iona, where the base of the sedimentary series is found resting on the Archæan gneiss.

doubtful impressions and the casts of worms. The fossiliferous bands are to be more thoroughly searched this summer, and it is hoped that something more determinable may be obtained from them.

Nevertheless, indistinct though these relics undoubtedly are, they may claim the interest which arises from their being at present the very oldest traces of organised existence yet found within our islands. Murchison classed the red sandstones of western Sutherland and Ross as "Cambrian," inasmuch as he found them to underlie unconformably strata containing what he believed to be Lower Silurian fossils. It is not improbable, however, that they belong to an older time than any of the Cambrian rocks of Wales.

That the red sandstones of the north-west of Scotland were laid down in shallow water seems to be clearly indicated by their current-bedding and ripple-marks, as well as by the occurrence of bands of conglomerate among them on many successive horizons. Yet they retain these characters throughout a depth of some 10,000 feet. We can walk over their edges and count every successive stratum for a thickness of more than 3000 feet along the sides of a single mountain. How, then, could such a continuous mass of shallow-water deposits be accumulated? I am not sure that any wholly satisfactory answer can be given to this question, which is one that arises in the investigation of various epochs of geological history. That the basins must have been due to local subsidence can hardly be doubted. We may suppose that this downward movement continued at the same time that the ridges which bounded the hollows continued to be forced upward. New shore-lines would thus be brought to the level of the water, and coarse shingle might be swept down upon previously deposited fine sediment. If occasionally the barrier between the basins and the open sea were partially submerged, the muddy ferruginous water of the enclosed tracts might be cleared out and the denizens of the sea might for a time enter them. Possibly the grey and dark shales may mark these irruptions of the ocean.

That similar conditions of geography prevailed at that period in the extreme north-west of Europe is indicated by the fact that in Norway a group of red sandstones and conglomerates known as the "Sparagmite rocks" is interposed between the Archæan gneiss and the oldest of the fossiliferous formations. In these Scandinavian rocks we probably see traces of the extension of similar enclosed water-basins along the eastern border of the primeval Atlantic Ocean northwards among the hollows of the Archæan land.

Before the next great geological period these basins had been entirely effaced, and the geography of the region had wholly changed. This transformation is probably traceable to two causes. First, the terrestrial movements which led to the formation and continuance of the basins may in the end have caused their extinction by raising them into land, and possibly at the same time by folding and fissuring their accumulated deposits. Secondly, as soon as these deposits, whether split open or not, were exposed to the atmosphere they would

begin to be worn down. That erosion took place during a prolonged period, and to a vast extent, is shown by the fact that in some places the thick cake of sandstone was hollowed out down to the Archæan platform below it before the next succeeding formations were deposited. Here again we are presented with a striking example of the imperfection of the Geological Record.

III. The Silurian Period.

After the long interval of time represented by the elevation of the red sandstones into dry land, and their entire removal from some places by denudation, the north-west of Scotland, and probably a large tract lying around it, sank under the sea. The depression seems to have been slow and gradual, and to have continued until the site of the Cambrian basins and of the surrounding region was covered with a considerable depth of clear open sea-water. The records of this subsidence are contained in a series of strata having a total thickness of somewhere about 2000 feet, and divisible into two chief groups—a Lower, composed of quartzites, grits, and thin conglomerate, about 500 feet in total depth, and an Upper, consisting almost wholly of limestone. Perhaps the most striking feature in this series of stratified rocks is the abundance of their organic remains. The quartzites are crowded with the tubes formed by sea-worms when the material existed as soft white sand on the sea-bottom. The limestones are made up of the remains of calcareous organisms, among which the most conspicuous that now remain are chambered shells and gasteropods. Throughout these limestones worm-casts are present almost everywhere, and in such abundance as to show, as Mr. Peach has pointed out, that "nearly every particle of the calcareous mud must have passed through the intestines of worms." A large collection of fossils has been made by the Geological Survey from these limestones, which, though not yet specifically determined, amply confirm the original generalisation of Salter, made more than thirty years ago, that the aspect or facies of organic remains in the limestones of the north-west of Scotland resembles that of the older parts of the Lower Silurian formations of Canada rather than that of the corresponding rocks in Wales. So marked is the resemblance to the American type as to indicate that some shore-line must once have stretched across the North Atlantic, in order to afford a platform for the free migration of marine life between the two areas. The contrast with the Welsh type has been explained by the probable existence of some barrier that separated the sea-bed over the north-west of Scotland from that of southern Scotland, England, and Wales. That such a barrier existed is tolerably certain, and I shall presently refer to some indications of its probable position. At the same time it may be open to question whether the Durness limestones can be properly correlated as homotaxial equivalents of any Lower Silurian rocks in Wales. My own impression is that they may be older than the

oldest Arenig rocks, and may be equivalent to some part of the "Primordial Silurian" or Cambrian series. This, however, is a question that must remain unsettled until a thorough critical examination of the fossils has been completed.

The area within which these Silurian quartzites and limestones can be certainly recognised, forms a narrow belt extending for about 110 miles along the north-west coast of Scotland, from the northern coast of Sutherland to the south of the island of Skye. Throughout that extent of ground the rocks exhibit remarkable persistence in the character and thickness of their several subdivisions, whence the inference may legitimately be drawn that the area within which they are now visible forms but a small part of the region over which they were originally deposited.

It was claimed by Murchison, and generally conceded by geologists, that the quartzites and limestones of the north-west pass upward into a younger series of schists, representing metamorphosed sedimentary rocks. This order of succession appeared to be established by the evidence of many clear natural sections along the whole tract from Durness to Skye. It was first adopted and afterwards opposed by Nicol, who in his later papers maintained that the supposed younger schists were merely the old or Archæan gneiss brought up again by great faults, and pushed over the younger formations. But he failed to account for the striking difference in petrographical character between the old gneiss and the younger schists, and for the remarkable coincidence between the general dip of the latter and that of the Silurian stratified rocks on which they seemed to rest conformably. During the last ten years various geologists have renewed the investigation of the question, among whom I may specially mention Dr. Hicks, Professor Bonney, Dr. Callaway, Professor Lapworth, and the members of the Geological Survey, particularly Messrs. Peach, Horne, and Clough. The result of their labours has been, in the first place, the discovery of one of the most complicated pieces of geological structure at present known in any country; in the second place, the abandonment of all further controversy, and the attainment of complete harmony regarding the order of geological succession in the north-west Highlands.

Murchison's view that there is a regular upward passage from the quartzites and limestones into the upper schists is proved to be erroneous, while Nicol's contention that the old gneiss is brought up again above the Silurian rocks is found to be so far correct. But the structure is now seen to be infinitely more complex than Nicol imagined, while, on the other hand, Murchison's belief that the younger schists were evidence of a gigantic metamorphism later than Lower Silurian time is undoubtedly true, though in a sense very different from that in which he looked at the question.

Nowhere in the north-west Highlands can any rock be seen resting in its original and natural position above the limestones. The highest limestone of Durness is the youngest rock of that region about the

geological position of which there is any certainty. At present we know absolutely nothing of other sedimentary strata which followed that limestone. That such strata continued to be deposited is certain, for the changes which the quartzites and limestones have undergone could not have taken place save under the pressure of a thick mass of overlying material. But this superincumbent mass has been entirely obliterated in the extraordinary series of terrestrial movements which I have now to describe.

IV. The Period of the Younger Schists.

Without entering into details, which are only intelligible with the help of a large map and sections, and even with this aid involve much disquisition of a technical kind, I may briefly say that after the deposition of the limestone and of the missing strata, whatever these may have been, which covered them, the whole region was convulsed by a series of disturbances, to which there has since been no parallel within our borders. By a series of intermittent movements the terrestrial crust, for thousands of feet downward, over the north-west Highlands, was fissured and pushed bodily westward. The various geological formations of that district—Archæan, Cambrian, and Silurian—were disrupted and driven over each other. Thus masses of rock, not more than a few hundred feet thick, were piled up so as to appear multiplied tenfold. The youngest strata were doubled under the oldest, and large slices of the ancient Archæan gneiss were made to rest on the Silurian limestones.

Fortunately the strongly marked characters of the different members of the Silurian series, the striking contrast between them and the Cambrian sandstones and Archæan gneiss, and the manner in which all these rocks are now laid bare on coast cliffs and rugged hill-sides, have rendered possible the task of unravelling this labyrinthine structure. The large maps, on the scale of 6 inches to a mile, on which this structure has been worked out by the Geological Survey, are by far the most complicated which the Survey has yet produced; indeed, I am not aware that such mapping has ever before been attempted.—[Some specimens of these maps were exhibited.]

On exposed rock-faces we see a thin group of strata repeated again and again by small reversed faults, the lower beds being made to rest on the higher till they occupy a great breadth of ground, and appear of considerable thickness. Further examination will generally show that they have been all pushed westwards, and that their truncated under ends rest upon a platform of undisturbed rock along which they have travelled. We may further observe them to be abruptly cut off at a higher level by a sharp line, on which perhaps stands another series of piled-up beds. This piling up and truncation of the rocks is followed by a still more gigantic displacement. Lower and lower portions of the geological series have been torn up and thrust westward until at last the Archæan platform has given

way, and masses of it, many hundreds of feet in thickness and many miles in length, have been driven over the younger formations. The horizontal distance to which this removal has reached can sometimes be shown to have amounted to at least ten miles; perhaps it may have been sometimes even greater.

In studying this complicated system of dislocations we soon meet with evidence that the movements were not all effected at one time, but that on the contrary they took place at intervals, the earlier being disrupted by the later. The lines of maximum thrust override those of lesser size, and the most easterly of these lines passes successively across all the others till it rests directly on unmoved rocks. The period of terrestrial disturbance was probably a prolonged one, and this inference is strengthened by other evidence to be afterwards adduced.

The direction of movement has been on the whole from the E.S.E. Bordering the west coast of Sutherland and Ross there is a strip of ground about 10 or 15 miles broad and some 90 miles long, in which the rocks have not been displaced. East of that strip, along a belt of dislocation varying up to five or six miles in breadth, the disturbances become increasingly numerous and powerful towards the interior, until at last a gigantic thrust-plane is encountered, above and beyond which the rocks have been so crushed and altered, that it is for the most part no longer possible to tell what their original character has been. They are now flaggy schists—the younger "quartzose and gneissose flagstones" of Murchison, "the Moine schists" of the Geological Survey.

The enormous amount of fracturing, displacing and crushing caused by these terrestrial disturbances has resulted in the development of regional metamorphism on an extensive scale. Every stage can be traced from a sandstone or conglomerate into a perfect schist, and from the most typical coarse Archæan gneiss into a fine laminated slate.

Where the feeblest amount of alteration has taken place, the rock has been merely somewhat crushed, its larger crystals or pebbles have been fractured, and the separated portions have been recemented. A further stage is shown where the fine material of the rock has been more comminuted and has been drawn out round the flattened and elongated crystals or pebbles. The latter give way in proportion to their power of resistance. The felspars and hornblendes are first left as "eyes" and then crushed down till they disappear in the general matrix. The harder quartz-pebbles of the conglomerates have resisted longer; but they too, in the planes of great movement, are found to be pulled out to twice or four times their length, or to be flattened out into mere thin plates like pennies. One of the most singular proofs of this internal movement of the component particles of even so obdurate a rock as quartzite is shown by the deformation of the worm-tubes. As these tubes come within the influence of the movement their vertical position changes into an inclined one, and they

become gradually flatter and more drawn out till at last, before they cease to be traceable, they appear as mere long ribbons on the surface of the rock, which then becomes a quartz-schist. Along the planes of intense crushing the original structure of a rock is entirely effaced, its crystals or grains are ground into fragments, and it acquires a streaked laminated structure like a shale or slate.

But for the most part, concomitant with the mechanical destruction of the various rocks, there has been a chemical and mineralogical re-arrangement of their particles. Out of their broken-down materials new minerals have crystallised, and this process of reconstruction has, in the most thoroughly altered masses, proceeded so far that the whole new structure is now crystalline. In this manner, mica, quartz, felspar, hornblende, and other minerals, have been developed, and have arranged themselves along the lines of movement in the crushed rock. These lines, approximating to the surfaces of the great thrust-planes, may be utterly discordant from the structure-lines, such as those of foliation or bedding, in the original mass. Rocks of this character are true schists, and I know of no internal or external signs by which, apart from field-evidence, they are to be distinguished from Archæan schists as to the derivation of which we can only guess, and which, therefore, must in the meantime be considered as original rocks.

By the aid of the microscope, much assistance is obtained in tracing out the mineral transformations which have taken place in the course of this regional metamorphism. To show the larger features of the change, so far as they can be judged of in hand-specimens, I exhibit on the table a series of pieces of the crushed gneiss, quartzite, and conglomerate; and to illustrate the internal changes I show a selection of slides on the screen, photographed from thin slices of the rocks as seen under the microscope.

The importance of the discovery of this belt of extreme complication in the north-west Highlands can hardly be overestimated. It gives us the key to the geological structure, not only of the Highlands, but of all the areas of younger crystalline schists in our own area, and will doubtless be found to explain much in the geological structure of Scandinavia. The lines of maximum thrust-planes can be followed for 100 miles, from the north of Sutherland into Skye; but this is only a small part of their extent. They can be picked up again in the west of Mayo and Donegal, a total distance of some 400 miles. That similar lines of movement have affected Scandinavia and produced the distinctive strike of the rocks there can hardly be doubted, so that the total length of disturbed country in north-western Europe probably exceeds 1600 miles, trending in a general north-north-east direction.

How far the influence of the great terrestrial movements extended eastwards from what now appears as the belt of maximum disturbance, and what effect it had upon the configuration of the surface, are questions to which as yet no satisfactory answer can be given.

It is difficult to suppose that such colossal displacements and fractures of the crust should not have powerfully affected the superficial topography of the time. They may have produced a high mountain range, or a succession of parallel ranges, extending along the north-west of Europe. The existence of some such mass of land is needed to account for the vast piles of sediment of which the Palæozoic, Secondary, and Tertiary formations have been built up. So great, however, is the antiquity of these terrestrial movements, so continual and gigantic has been the denudation, and so repeated have been the oscillations of level, that the upheaved land has been reduced to the fragments that now form the Highlands and Islands of the west of Ireland, of Scotland, and of Scandinavia.

It is quite clear that during the disturbances in the north-west region the main thrust came from the eastward. It will be interesting to discover how far towards the east these disturbances affected the structure of the rocks beneath. That it reached across the whole breadth of the Scottish Highlands, that is for a distance of 100 to 130 miles, can be conclusively proved. That it extended much further and embraced within its area the whole of the Silurian regions of the three kingdoms can, I think, be shown to be highly probable.

To understand this part of the problem it is necessary to consider the structure of the ground immediately to the east of the belt of extreme complication in the north-west Highlands. I have said that the displacements and metamorphism increased in intensity from west to east, until at last, by a final gigantic thrust, a series of reconstructed schists has been driven over rocks whose origin can still be determined. Among these eastern schists it is occasionally possible to detect more or less reliable traces of the original rocks out of the crushing down of which they have been formed. Thus we find in the northern part of the area slices of Archæan and eruptive rocks, and in the south an increasing amount of material which has been derived from the destruction of the red Cambrian sandstones. It is tolerably evident that in the broad band of country which extends from the belt of complication eastwards to the Moray Firth and the line of the Great Glen, and embraces the mountainous tracts of Sutherland, Ross, western Inverness-shire, and north-western Argyllshire, the lower parts of the Geological Record are repeated again and again. It is mainly the Archæan platform, with its covering of Cambrian sandstones, and possibly the lower parts of the Silurian series, which have been broken up, plicated, crushed, and converted into the series of crystalline schists that form the picturesque heights of Ben Hope and Ben Klibric southward to Moidart and Morven. Nevertheless when this wild tract of country comes to be mapped out in detail, there will probably be found intercalated bands of higher formations which have here and there been caught in folds of the lower rocks.

But when we pass eastwards from the Great Glen into the mountains of eastern Inverness-shire, Perthshire, and the south-western

Highlands, we encounter a totally different series of rocks. Though greatly plicated, dislocated, crushed, and metamorphosed, these rocks can be recognised as unquestionably, in the main, of sedimentary origin. They must be many thousands of feet in thickness, including among their members such rocks as conglomerate, pebbly grit, quartzite, black slate, andalusite slate, phyllite, mica-schist, fine flaggy gneiss, and limestone, together with intrusive sheets and bosses of various eruptive rocks. Some of the groups of this series can be followed and mapped for long distances with nearly as much ease as the members of a succession of unaltered Palæozoic or Secondary formations. There is a belt of limestone, for example, which has been traced by the Geological Survey almost continuously from the coast of Banffshire to the west of Argyllshire, through the very heart of the Highlands—a total distance of not much less than 200 miles. These limestones have for the most part become so thoroughly crystalline that fossils can hardly be expected to be found in them, though there are occasional less altered portions of rock which may eventually prove to be fossiliferous. The limestones are associated with quartzites and schists, as unaltered limestones are with sandstones and shales. I cannot myself doubt that they have been formed by the aggregation of the remains of calcareous organisms. The same rocks are prolonged into the north of Ireland, where one of the dark limestones at Culdaff has lately yielded certain bodies which some palæontologists have declared to be the remains of a coral (*Favosites*). The black slates which so closely resemble the dark carbonaceous shales of the Lower Silurian region of south Scotland have afforded in Donegal some curious pyritous markings strongly suggestive of graptolites.

Out of this enormous mass of metamorphosed sedimentary strata the Scottish Highlands east of the Great Glen are built up, as well as the region which extends southwards across the north-west of Ireland as far as the centre of County Galway. The first question that requires an answer with regard to it has reference to its relation to the fossiliferous quartzites and limestones of the north-west. Murchison, who led the way in the investigation of the stratigraphy of the Highlands, believed that the quartzites and limestones of the Central Highlands lay towards the base of the whole series of post-Cambrian rocks, and were the south-eastward extensions of those of Sutherland. But recent investigations throw some doubt on this view, which at the time it was promulgated seemed so natural and simple. We know that the quartzites and limestones of the Central Highlands, so far from being near the bottom of the vast series of schists, are underlain by many thousand feet of other metamorphosed sedimentary strata, and that the actual base is nowhere reached in that region.

During the last two years, in concert with some of my colleagues of the Geological Survey, I have devoted some time to the task of endeavouring to find the bottom of these crystalline schists of Scot-

land and Ireland, as a necessary foundation for placing them on their true geological horizon. and at length this spring our efforts have been successful beyond our expectations. Last year in the north-west of the island of Islay I found a group of scarcely altered shales, grits, and thin limestones emerging from beneath the black slates which underlie the schists, limestones, and quartzites of that region. So little have these strata suffered from the metamorphism which has affected the rocks lying above and to the east of them, that I quite anticipate that fossils will be found in them. This year, in company with Mr. C. T. Clough, I came upon a somewhat similar group of little metamorphosed black slates and grits at the north-east end of the island of Iona. I am hopeful that these strata will yield fossils; I myself found in them some short black lines, which at once recalled the form and condition of the fragments of the central rods of graptolites so commonly met with in the black shales of the Southern Uplands of Scotland. The discovery of recognisable fossils in these strata would fix the geological age of the rocks of the Central Highlands and of the north-west of Ireland.

An interesting feature about these slates of Iona is that they lie at the very bottom of the series of younger schists. Immediately under them are a coarse grit (arkose) and conglomerate, formed out of the Archæan gneiss, which comes out in great force from underneath them and forms the main part of the island.* The uprise of an axis of the old gneiss so far to the east of the line of great complication, and at the base of the vast sedimentary masses of the Central Highlands, is a fact of great importance. We seem to find here a fragment of the old barrier which separated the American province in which the Durness limestones were deposited, from the area of Western and Central Europe in which the other Silurian formations of Britain were laid down. Prolongations of the same ridge towards the north-east are possibly to be traced even as far as the mountains between the head of the rivers Nairn and Findhorn, where some of my colleagues think that there is probably another core of the Archæan gneiss.

The search for a base to the same great series of schists as they are developed in the north-west of Ireland has been equally successful. Along the west of County Mayo Archæan gneiss has been recognised by us,† exhibiting the typical characters of the same rock in the north-west of Scotland. In Achill Island we found the base of the quartzite and schist series in the form of a coarse quartz-conglomerate resting on the gneiss. But all these rocks have come within the influence of the intense regional metamorphism. The conglomerate in particular has had its quartz-pebbles pulled out in

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* The existence of a slight displacement at the actual junction does not obscure the evidence of the true relation of the rocks.

† In my recent traverses in the west of Ireland I had the advantage of the company and assistance of my colleagues, Mr. Peach, Mr. M'Henry, and Dr. Hyland.

the direction of movement, and its paste has been converted into a fine kind of gneiss.

Having thus traced an original westward boundary to the younger crystalline schists of Ireland and Scotland, I saw that it would be important to follow their eastern boundary as far as it had not been concealed by later formations. In Galway we found that the quartzites, limestones, and schists are succeeded to the south by the large area of Archæan gneiss already referred to. But the boundary between the two groups of rock is one of extreme complication, somewhat like that of the north-west Highlands. Along a line running east and west through the heart of this county from Mannin Bay to Lough Corrib, the two groups have been so dislocated and so thrust between and over each other that much time and patience, with the use of large-scale maps, would be required to map out their respective areas. But the important fact is readily perceptible that in Galway the uprise of a large Archæan area gives us a southern limit for the basin in which the younger schists of the north-west of Ireland were deposited.

To the east and north-east of the Galway area the country has been overspread with Old Red Sandstone and Carboniferous strata, so that for a long space the older rocks are concealed. Far to the north-east in Tyrone, on the southern borders of the great area of crystalline schists, a mass of dark hornblendic rocks was mapped some years ago by Mr. Nolan of the Geological Survey of Ireland, and referred doubtfully to a pre-Cambrian age. A more recent examination of this mass, with the experience gained over so wide a region among the older crystalline rocks, has enabled us to identify it without hesitation as a characteristic portion of the Archæan gneiss. It rises as a long north-east ridge along the south-eastern margin of the chloritic schists of Londonderry which were deposited against and over it. We discovered moreover that these schists have at their base, resting on the old gneiss, a thick volcanic series consisting of amygdaloidal basic lavas, tuffs, and coarse volcanic agglomerates. The green chloritic material of the schists, not improbably represents the original magnesian silicates in the finer volcanic dust that mingled with the ordinary sediment of the sea-bottom.

From the evidence now adduced, it is, I think, manifest that the crystalline schists of the Scottish Highlands east of the Great Glen, as well as their continuation into the north-west of Ireland, cannot be regarded as merely the equivalents of the quartzites and limestones of Sutherland and Ross. They are enormously thicker and more varied in their component members than those north-western strata. Whether even any part of them represents the sedimentary rocks of the north-west seems to me open to serious doubt. My own impression is that they are probably younger than these rocks, and that they once stretched far to the north-west and covered them to a depth of many thousands of feet. That the fossiliferous strata of the north-west Highlands were originally buried under a thick pile of other sediments I have already shown.

The last question on which I propose to touch is the geological date of the extraordinary terrestrial disturbances to which the crystalline schists of the Highlands of Scotland and the north-west of Ireland, owe their characteristic structures. The limit of its antiquity is easily fixed. As these disturbances involve rocks containing fossils of Lower Silurian age, they must obviously have taken place after some part at least of the Lower Silurian period. In Scotland their chronological limit in the other direction is determined by the fact that the conglomerates of the Lower Old Red Sandstone, are largely composed of the crystalline schists of the Highlands. They must consequently have occurred before the deposition of some part at least of the Lower Old Red Sandstone. Here, then, is a long geological interval within which the gigantic upthrusts and metamorphism began and ended.

But the evidence obtained in Ireland enables us to fill up this interval with a little more definiteness. In southern Mayo and northern Galway, as Professor Hull has pointed out, the Upper Silurian rocks rest upon and contain abundant fragments of the younger crystalline schists which stretch into these counties from Donegal. And the inference has naturally been drawn that the great terrestrial disturbances and metamorphism occurred before the Upper Silurian period. But a recent more critical examination of the ground has satisfied me that this inference, though to a certain extent correct, does not embrace the whole truth.

Those who have visited Connemara may remember the singular group of mountains, which hem in the Killary fjords, and rise in Mweelrea and its neighbouring ridges to a height of more than 2600 feet above the sea that frets their base. These massive buttresses of rock owe their distinctive forms to the thick beds of coarse grit and conglomerate of which they are in great measure built up. An abundant series of fossils proves that this mass of deposits is of Upper Silurian age. It is the base of these exceedingly coarse sediments which along their southern margin can be seen to rest upon the upturned edges of the crystalline schists, and to be there largely made up of fragments derived from that metamorphic platform. The numerous bands of coarse conglomerate upon successive horizons serve to indicate considerable terrestrial disturbance during their deposition. That the commotion continued after that time is further shown by the remarkable way in which the rocks have been dislocated. These Upper Silurian sediments have been broken up into large mountainous blocks which have been thrown on end or actually pushed over each other. So violent has the movement been along certain lines, that the bands of greywacke and shale have been intensely crumpled and puckered, and have actually been converted locally into fine micaceous schists.

Hence it seems tolerably certain that though in the west of Ireland the chief plications, fractures, and metamorphism were completed before Upper Silurian time, and though a vast interval

must have elapsed during which the progress of denudation laid bare the younger schists and thereby provided materials for the Upper Silurian conglomerates, the terrestrial disturbances nevertheless continued during the deposition of these conglomerates, and were renewed with increased vigour afterwards.

If we compare the geological structure of the Silurian tracts of England, Wales, the south of Scotland, and the east of Ireland, with that of the areas of the younger crystalline schists, many points of resemblance will be seen to occur between them. Towards the north and north-west we find that the Archæan, Cambrian and oldest Silurian rocks, now exposed there by the progress of denudation, have been subjected to the intensest mechanical deformation, and have assumed the most completely schistose structures. Coming southward, we trace the younger crystalline schists of the central Highlands and of Donegal thrown into innumerable north-east and south-west folds, and becoming less and less metamorphosed as they are followed towards the lower grounds. Still further south the Lower and Upper Silurian rocks, plicated, crumpled and dislocated, repeat the familiar structure of the southern Highlands, but with only partial and feeble metamorphism. I am disposed to look upon the whole of these structures as the result of one great succession of terrestrial movements which began and reached their maximum of intensity during some part of Lower Silurian time, but which continued to repeat themselves at intervals with greater or less vigour through a long series of geological ages, down to the early part of the Old Red Sandstone period.

As the consequence of this prolonged disturbance the Archæan and older Palæozoic rocks have been thrown into those north-east and south-west folds, which have in large part determined the trend of the land in the north-west of Europe. The shaping of our mountains into their present forms has been brought about by ages of subsequent sculpture in which the agencies employed by nature have operated mainly on the surface, but the carving of their features has been guided by the internal structures developed by those subterranean movements which we have been considering.

[A. G.]

WEEKLY EVENING MEETING,

Friday, June 14, 1889.

William Huggins, Esq. D.C.L. LL.D. F.R.S. Vice-President, in the Chair.

C. V. Boys, Esq. F.R.S.

Quartz Fibres.

In almost all investigations which the physicist carries out in the laboratory, he has to deal with, and to measure with accuracy, those subtle, and, to our senses, inappreciable forces to which the so-called laws of nature give rise. Whether he is observing by an electrometer the behaviour of electricity at rest, or by a galvanometer the action of electricity in motion; whether in the tube of Crookes he is investigating the power of radiant matter, or by the famous experiment of Cavendish he is finding the mass of the earth—in these and in a host of other cases he is bound to measure, with certainty and accuracy, forces so small that in no ordinary way could their existence be detected; while disturbing causes, which might seem to be of no particular consequence, must be eliminated if his experiments are to have any value. It is not too much to say that the very existence of the physicist depends upon the power which he possesses of producing at will, and by artificial means, forces against which he balances those that he wishes to measure.

I had better, perhaps, at once indicate in a general way the magnitude of the forces with which we have to deal.

The weight of a single grain is not to our senses appreciable, while the weight of a ton is sufficient to crush the life out of any one in a moment. A ton is about 15,000,000 grains. It is quite possible to measure, with unfailing accuracy, forces which bear the same relation to the weight of a grain that a grain bears to a ton.

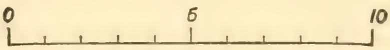
To show how the torsion of wires or threads is made use of in measuring forces, I have arranged what I can hardly dignify by the name of an experiment. It is simply a straw hung horizontally by a piece of wire. Resting on the straw is a fragment of sheet-iron weighing ten grains. A magnet, so weak that it cannot lift the iron, yet is able to pull the straw round through an angle so great that the existence of the feeble attraction is evident to every one in the room.

Now it is clear that if, instead of a straw moving over the table simply, we had here an arm in a glass case and a mirror to read the motion of the arm, it would be easy to observe a movement a hundred or a thousand times less than that just produced, and, therefore, to

measure a force a hundred or a thousand times less than that exerted by this feeble magnet.

Again, if instead of wire as thick as an ordinary pin, I had used the finest wire that can be obtained, it would have opposed the movement of the straw with a far less force. It is possible to obtain wire ten times finer that this stubborn material, but wire ten times finer is much more than ten times more easily twisted. It is ten thousand times more easily twisted. This is because the torsion varies as the fourth power of the diameter, so we say $10 \times 10 = 100$; $100 \times 100 = 10{,}000$. Therefore, with the finest wire, forces 10,000 times feebler still could be observed.

It is, therefore, evident how great is the advantage of reducing the size of a torsion wire. Even if it is only halved, the torsion is



Scale of 1000ths of an inch for Figs. 1 to 7. The scale of Figs. 8 and 9 is much finer.

FIG. 1.

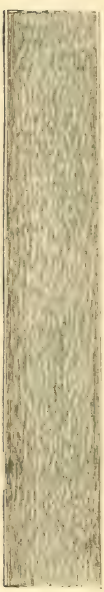
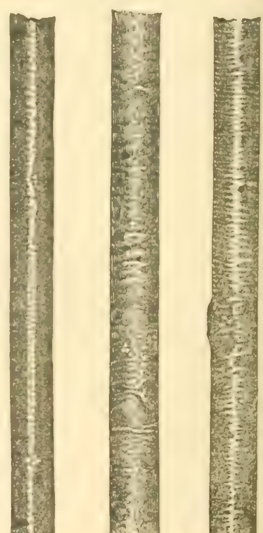


FIG. 2.

FIG. 3.



reduced sixteen-fold. To give a better idea of the actual sizes of such wires and fibres as are in use, I shall show upon the screen a series of photographs taken by Mr. Chapman, on each of which a scale of thousandths of an inch has been printed.

The first photograph (Fig. 1) is an ordinary hair—a sufficiently

familiar object, and one that is generally spoken of as if it were rather fine. Much finer than this is the specimen of copper wire now on the screen (Fig. 2), which I recently obtained from Messrs. Nalder Brothers. It is only a little over one-thousandth of an inch in diameter. Ordinary spun glass, a most beautiful material, is about one-thousandth of an inch in diameter, and this would appear to be an ideal torsion thread (Fig. 3). Owing to its fineness, its torsion would be extremely small, and the more so because glass is more easily deformed than metals. Owing to its very great strength, it can carry heavier loads than would be expected of it. I imagine many physicists must have turned to this material in their endeavour to find a really delicate torsion thread. I have so turned, only to be disappointed. It has every good quality but one, and that is its imperfect elasticity. For instance, a mirror hung by a piece of spun glass is casting an image of a spot of light on the scale. If I turn the mirror, by means of a fork, twice to the right, and then turn it back again, the light does not come back to its old point of rest, but oscillates about a point on one side, which, however, is slowly changing, so that it is impossible to say what the point of rest really is. Further, if the glass is twisted one way first, and then the other way, the point of rest moves in a manner which shows that it is not influenced by the last deflection alone: the glass remembers what was done to it previously. For this reason spun glass is quite unsuitable as a torsion thread; it is impossible to say what the twist is at any time, and, therefore, what is the force developed.

Fig. 4.

So great has the difficulty been in finding a fine torsion thread, that the attempt has been given up, and in all the most exact instruments silk has been used. The natural cocoon fibres, as shown on the screen (Fig. 4), consist of two irregular lines gummed together, each about one two-thousandth of an inch in diameter. These fibres must be separated from one another and washed. Then each component will, according to the experiments of Gray, carry nearly 60 grains before breaking, and can be safely loaded with 15 grains. Silk is, therefore, very strong, carrying at the rate of from 10 to 20 tons to the square inch. It is further valuable in that its torsion is far less than that of a fibre of the same size of metal or even of glass, if such could be produced. The torsion of silk, though exceedingly small, is quite sufficient to upset the working of any delicate instrument, because it is never constant. At one time the fibre twists one way, and at another time another, and the evil effect can only be mitigated by using large apparatus in which strong forces are developed. Any attempt that may be made to increase the delicacy of apparatus by reducing their dimensions is at once prevented by the relatively great importance of the vagaries of the silk suspension.

The result, then, is this. The smallness, the length of period, and therefore delicacy, of the instruments at the physicist's disposal have until lately been simply limited by the behaviour of silk. A more perfect suspension means still more perfect instruments, and therefore advance in knowledge.

It was in this way that some improvements that I was making in an instrument for measuring radiant heat came to a deadlock about two years ago. I would not use silk, and I could not find anything else that would do. Spun glass, even, was far too coarse for my purpose; it was a thousand times too stiff.

Fig. 5.

There is a material, invented by Wollaston long ago, which, however, I did not try, because it is so easily broken. It is platinum wire which has been drawn in silver, and finally separated by the action of nitric acid. A specimen about the size of a single line of silk is now on the screen, showing the silver coating at one end (Fig. 5).

As nothing that I knew of could be obtained that would be of use to me, I was driven to the necessity of trying by experiment to find some new material. The result of these experiments was the development of a process of almost ridiculous simplicity, which it may be of interest for me to show.

The apparatus consists of a small cross-bow, and an arrow made of straw with a needle point. To the tail of the arrow is attached a fine rod of quartz, which has been melted and drawn out in the oxyhydrogen jet. I have a piece of the same material in my hand, and now, after melting their ends and joining them together, an operation which produces a beautiful and dazzling light, all I have to do is to liberate the string of the bow by pulling the trigger with one foot, and then, if all is well, a fibre will have been drawn by the arrow, the existence of which can be made evident by fastening to it a piece of stamp-paper.

In this way threads can be produced of great length, of almost any degree of fineness, of extraordinary uniformity, and of enormous

strength. I do not believe, if any experimentalist had been promised by a good fairy that he might have anything he desired, that he would have ventured to ask for any one thing with so many valuable properties as these fibres possess. I hope, in the course of this evening, to show that I am not exaggerating their merits.

In the first place, let me say something about the degree of fineness to which they can be drawn. There is now projected upon the screen a quartz fibre one five-thousandth of an inch in diameter (Fig. 6). This is one which I had in constant use in an instrument and carrying about 30 grains. It has a section only one-sixth of that of a single line of silk, and it is just as strong. Not being organic, it is in no way affected by changes of moisture and temperature, and so it is free from the vagaries of silk which give so much trouble. The piece used in the instrument was about 16 inches long. Had it been necessary to employ spun glass, which hitherto was the finest torsion material, then, instead of 16 inches, I should have required a piece 1000 feet long, and an instrument as high as the Eiffel tower to put it in.

FIG. 6.

FIG. 7.

There is no difficulty in obtaining pieces as fine as this yards long if required, or in spinning it very much finer. There is upon the screen a single line made by the small garden spider, and the size of this is perfectly evident (Fig. 7). You now see a quartz fibre far finer than this, or, rather, you see a diffraction phenomenon, for no true image is formed at all; but even this is a conspicuous object in comparison with the tapering ends, which it is absolutely impossible to trace in a microscope. The next two photographs, taken by Mr. Nelson, whose skill and resources are so famous, represent the extreme end of a tail of quartz, and though the scale is a great deal larger than that used in the other photographs, the end will be visible only to a few. Mr. Nelson has photographed here what it is absolutely impossible to see. What the size of these ends may be, I have no means of telling. Dr. Royston Piggott has estimated some of them at less than one-millionth of an inch, but whatever they are, they supply for the first time objects of extreme smallness, the form of which is certainly known, and therefore I cannot help looking upon them as more satisfactory tests for the microscope than diatoms and other things of the real shape of which we know nothing whatever.

Since figures as large as a million cannot be realised properly, it may be worth while to give an illustration of what is meant by a fibre one-millionth of an inch in diameter.

A piece of quartz an inch long and an inch in diameter would, if drawn out to this degree of fineness, be sufficient to go all the way round the world 658 times; or a grain of sand, just visible—that is, one-hundredth of an inch long and one-hundredth of an inch in

diameter—would make 1000 miles of such thread. Further, the pressure inside such a thread, due to a surface tension equal to that of water, would be 60 atmospheres.

Going back to such threads as can be used in instruments, I have made use of fibres one ten-thousandth of an inch in diameter, and with these the torsion is 10,000 times less than that of spun glass.

As these fibres are made finer, their strength increases in proportion to their size, and surpasses that of ordinary bar steel, reaching, to use the language of engineers, as high a figure as 80 tons to the inch. Fibres of ordinary size have a strength of 50 tons to the inch.

While it is evident that these fibres give us the means of producing an exceedingly small torsion, and one that is not affected by weather, it is not yet evident that they may not show the same fatigue that makes spun glass useless. I have therefore a duplicate apparatus with a quartz fibre, and you will see that the spot of light comes back to its true place on the screen after the mirror has been twisted round twice.

I shall now for a moment draw your attention to that peculiar property of melted quartz that makes threads such as I have been describing a possibility. A liquid cylinder, as Plateau has so beautifully shown, is an unstable form. It can no more exist than can a pencil stand on its point. It immediately breaks up into a series of spheres. This is well illustrated in that very ancient experiment of shooting threads of resin electrically. When the resin is hot, the liquid cylinders which are projected in all directions break up into spheres, as you see now upon the screen. As the resin cools, they begin to develop tails; and when it is cool enough, i.e. sufficiently viscous, the tails thicken, and the beads become less, and at last uniform threads are the result. The series of photographs show this well.

There is a far more perfect illustration, which we have only to go into the garden to find. There we may see in abundance what is now upon the screen—the webs of those beautiful geometrical spiders. The radial threads are smooth, like the one you saw a few minutes ago, but the threads that go round and round, are beaded. The spider draws these webs slowly, and at the same time pours upon them a liquid, and still further to obtain the effect of launching a liquid cylinder in space, he, or rather she, pulls it out like the string of a bow, and lets it go with a jerk. The liquid cylinder cannot exist, and the result is what you now see upon the screen (Fig. 8). A more perfect illustration of the regular breaking up of a liquid cylinder, it would be impossible to find. The beads are, as Plateau showed they ought to be, alternately large and small, and their regularity is marvellous. Sometimes two still smaller beads are developed, as may be seen in the second photograph, thus completely agreeing with the results of Plateau's investigations.

I have heard it maintained that the spider goes round her web

and places these beads there afterwards. But since a web with about 360,000 beads is completed in an hour—that is, at the rate of about 100 a second—this does not seem likely. That what I have said is true, is made more probable by the photograph of a beaded web that I have made myself by simply stroking a quartz fibre with a straw wetted with castor-oil (Fig. 9). It is rather larger than a spider line; but I have made beaded threads, using a fine fibre, quite indistinguishable from a real spider web, and they have the further similarity that they are just as good for catching flies.

FIG. 8.

FIG. 9.

Now, going back to the melted quartz, it is evident that if it ever became perfectly liquid, it could not exist as a fibre for an instant. It is the extreme viscosity of quartz, at the heat even of an electric arc, that makes these fibres possible. The only difference between quartz in the oxyhydrogen jet, and quartz in the arc, is that in the first you make threads, and in the second are blown bubbles. I have in my hand some microscopic bubbles of quartz, showing all the perfection of form and colour that we are familiar with in the soap bubble.

An invaluable property of quartz is its power of insulating perfectly, even in an atmosphere saturated with water. The gold leaves now diverging, were charged some time before the lecture, and hardly show any change, yet the insulator is a rod of quartz only three-quarters of an inch long, and the air is kept moist by a dish of water. The quartz may even be dipped in the water, and replaced with the water upon it, without any difference in the insulation being observed.

Not only can fibres be made of extreme fineness, but they are wonderfully uniform in diameter. So uniform are they, that they perfectly stand an optical test so severe that irregularities invisible in any microscope would immediately be made apparent. Every one must have noticed, when the sun is shining upon a border of flowers and shrubs, how the lines which the spiders use as railways to travel upon from place to place glisten with brilliant colours. These colours are only produced when the fibres are sufficiently fine. If you take one of these webs and examine it in the sunlight, you will find that the colours are variegated, and the effect consequently is one of great beauty.

The quartz fibre of about the same size shows colours in the same way, but the tint is perfectly uniform on the fibre. If the colour of the fibre is examined with a prism, the spectrum is found to consist of alternate bright and dark bands. Upon the screen are photographs taken by Mr. Briscoe, a student in the laboratory at South Kensington, of the spectra of some of these fibres at different

angles of incidence. It will be seen that coarse fibres have more bands than fine, and that the number increases with the angle of incidence of the light. There are peculiarities in the march of the bands as the angle increases which I cannot describe now. I may only say that they appear to move not uniformly but in waves, presenting very much the appearance of the legs of a caterpillar walking.

So uniform are the quartz fibres, that the spectrum from end to end consists of parallel bands. Occasionally a fibre is found which presents a slight irregularity here and there. A spider line is so irregular that these bands are hardly observable; but, as the photograph on the screen shows, it is possible to trace them running up and down the spectrum when you know what to look for.

To show that these longitudinal bands are due to the irregularities, I have drawn a taper piece of quartz by hand, in which the two edges make with one another an almost imperceptible angle, and the spectrum of this shows the gradual change of diameter by the very steep angle at which the bands run up the spectrum.

Into the theory of the development of these bands I am unable to enter; that is a subject on which your Professor of Natural Philosophy is best able to speak. Perhaps I may venture to express the hope, as the experimental investigation of this subject is now rendered possible, that he may be induced to carry out a research for which he is so eminently fitted.

Though this is a subject which is altogether beyond me, I have been able to use the results in a practical way. When it is required to place into an instrument a fibre of any particular size, all that has to be done is to hold the frame of fibres towards a bright and distant light, and look at them through a low-angled prism. The banded spectra are then visible, and it is the work of a moment to pick out one with the number of bands that has been found to be given by a fibre of the desired size. A coarse fibre may have a dozen or more, while such fibres as I find most useful have only two dark bands. Much finer ones exist, showing the colours of the first order with one dark band; and fibres so fine as to correspond to the white, or even the gray, of Newton's scale, are easily produced.

Passing now from the most scientific test of the uniformity of these fibres, I shall next refer to one more homely. It is simply this; the common garden spider, except when very young, cannot climb up one of the same size as the web on which she displays such activity. She is perfectly helpless, and slips down like a bead upon a wire. After vainly trying to make any headway, she finally puts her hands (or feet) into her mouth, and then tries again, with no better success. I may mention that a male of the same species is able to run up one of these with the greatest ease, a feat which may perhaps save the lives of a few of these unprotected creatures when quartz fibres are more common.

It is possible to make any quantity of very fine quartz fibre without a bow and arrow at all, by simply drawing out a rod of quartz

over and over again in a strong oxyhydrogen jet. Then, if a stand of any sort has been placed a few feet in front of the jet, it will be found covered with a maze of thread, of which the photograph on the screen represents a sample. This is hardly distinguishable from the web spun by this magnificent spider in corners of greenhouses and such places. By regulating the jet and the manipulation, anything from one of these stranded cables to a single ultro-microscope line may be developed.

And now that I have explained that these fibres have such valuable properties, it will no doubt be expected that I should perform some feat with their aid which, up to the present time, has been considered impossible, and this I intend to do.

Of all experiments, the one which has most excited my admiration, is the famous experiment of Cavendish, of which I have a full-size model before you. The object of this experiment is to weigh the earth by comparing directly the force with which it attracts things with that due to large masses of lead. As is shown by the model, any attraction which these large balls exert on the small ones will tend to deflect this six-foot beam in one direction, and then if the balls are reversed in position, the deflection will be in the other direction. Now, when it is considered how enormously greater the earth is than these balls, it will be evident that the attraction due to them must in comparison be excessively small. To make this evident, the enormous apparatus you see had to be constructed, and then, using a fine torsion wire, a perfectly certain but small effect was produced. The experiment, however, could only be successfully carried out in cellars or specially protected places, because changes of temperature produced effects greater than those due to gravity.*

Now I have, in a hole in the wall, an instrument no bigger than a galvanometer, of which a model is on the table. The balls of the Cavendish apparatus, weighing several hundredweight each, are replaced by balls weighing $1\frac{3}{4}$ lb. only. The smaller balls of $1\frac{3}{4}$ lb. are replaced by little weights of 15 grains each. The 6-foot beam is replaced by one that will swing round freely in a tube three-quarters of an inch in diameter. The beam is, of course, suspended by a quartz fibre. With this microscopic apparatus, not only is the very feeble attraction observable, but I can actually obtain an effect eighteen times as great as that given by the apparatus of Cavendish, and, what is more important, the accuracy of observation is enormously increased.

The light from a lamp passes through a telescope lens, and falls on the mirror of the instrument. It is reflected back to the table, and thence by a fixed mirror to the scale on the wall, where it comes to a focus. If the mirror on the table were plane, the whole movement of the light would be only about eight inches, but the mirror is

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* Dr. Lodge has been able, by an elaborate arrangement of screens, to make this attraction just evident to an audience.—C. V. B.

convex, and this magnifies the motion nearly eight times. At the present moment the attracting weights are in one extreme position, and the line of light is quiet. I will now move them to the other position, and you will see the result—the light slowly begins to move, and slowly increases in movement. In forty seconds it will have acquired its highest velocity, and in forty more, it will have stopped at 5 feet $8\frac{1}{2}$ inches from the starting point, after which it will slowly move back again, oscillating about its new position of rest. It has moved up to and stopped exactly at the division indicated.

It is not possible at this hour to enter into any calculations; I will only say that the motion you have seen is the effect of a force of less than one ten-millionth of the weight of a grain, and that with this apparatus I can detect a force two thousand times smaller still. There would be no difficulty even in showing the attraction between two No. 5 shot.

And now, in conclusion, I would only say that if there is anything that is good in the experiments to which I have this evening directed your attention, experiments conducted largely with sticks, and string and straw and sealing-wax, I may perhaps be pardoned if I express my conviction that in these days we are too apt to depart from the simple ways of our fathers, and instead of following them, to fall down and worship the brazen image which the instrument-maker hath set up.

[C. V. B.]

NOTE.—I have since learnt that in 1811 M. Gaudin melted quartz and drew it out by hand into threads. I have given an abstract of his experiments in the 'Electrical Review' of July 19th.

WEEKLY EVENING MEETING,

Friday, June 8, 1888.

Sir Frederick Bramwell, D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Professor Dewar, M.A. F.R.S. *M.R.I.*

Phosphorescence and Ozone.

In spectroscopic observations the experimenter is often much puzzled by the phenomena presented in high vacua, and the perplexity is largely due to the fact that we are unacquainted with the chemical changes which take place under such conditions. Special apparatus has to be devised for the purpose of attempting to solve some of these questions. Friction, heat, light, and electricity, will stimulate certain bodies, and cause them to become phosphorescent, and cooling the body may prevent the continuance of the luminosity. Again, by cooling the centre of a plate which has been coated with sulphide of calcium, light will make it phosphorescent everywhere but in the place it has been cooled. Heat increases the luminosity at first, but it afterwards dies out more quickly than where the plate has not been heated.

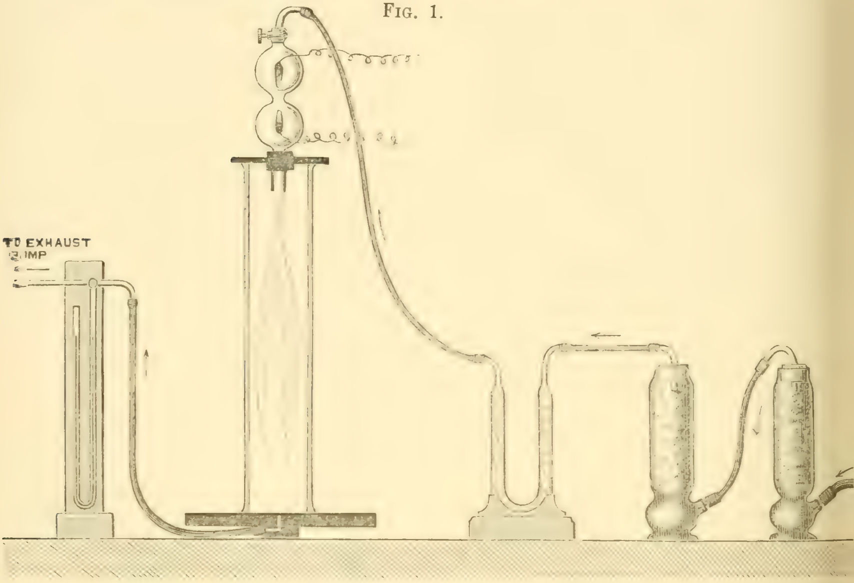
Geissler was the first to discover that phosphorescence is sometimes set up in residual gases in vacuum tubes. This was illustrated by sending a discharge through a series of vacuum bulbs, in which the traces of gas remained luminous for about five seconds after the discharge had ceased; when one of the bulbs was heated, on passing the discharge once more, that bulb alone remained dark. Becquerel and others investigated these phenomena; some of the inquirers came to the conclusion that they were produced only by oxygen compounds; others thought them to be due to some drying agent used in the construction of the bulbs.

Ozone is a very unstable body, which cannot be kept unless produced at a low temperature; its boiling-point is about -100° C., and at this temperature it is a blue liquid which exhibits high absorbent powers in the luminous part of the spectrum. At low temperatures substances may be dissolved in it, with which it explodes at high temperatures; bisulphide of carbon is one of these substances. On a former occasion I have shown that at -150° C. phosphorus does not combine with liquid oxygen, neither does sodium nor potassium, so that the absence of chemical combination between ozone and oxidisable substances is another proof of the negation of chemical combination at low temperatures. Smell is one of the most delicate tests of the presence of ozone, but inapplicable in the instance of the contents of a vacuum tube; the investigator has then to resort to chemical means and the study of the absorption spectra. In making ozone from oxygen, low pressures and the presence of moisture favour the

action, and that such conditions should favour chemical changes is contrary to what might have been expected.

In order to carry out the following experiments a good and powerful air-pump is required, and the Institution is fortunate in possessing a specially constructed instrument generously presented by the inventor, William Anderson, Esq. M.I.C.E. Director-General of Ordnance Factories.

FIG. 1.



Phosphorescent Gases Apparatus.

The more essential part of the apparatus is represented in Fig. 1. Common air is first dried and purified by passing through one vessel containing calcium chloride, and another containing caustic potash; the latter absorbs the carbonic acid. The air is next filtered by passage through a U-tube filled with cotton wool, after which it enters through a carefully adjusted small tap, the two-bulbed vacuum tube represented in the cut. The narrow channel between the bulbs is necessary; the glow is concentrated thereby, and this seems to have something to do with the effects obtained. It makes no difference whether platinum, charcoal, or aluminium poles be used inside the vacuum tube. The lower part of the tube opens into a tall glass vessel, connected below with the exhaust pump and a mercurial pressure gauge.

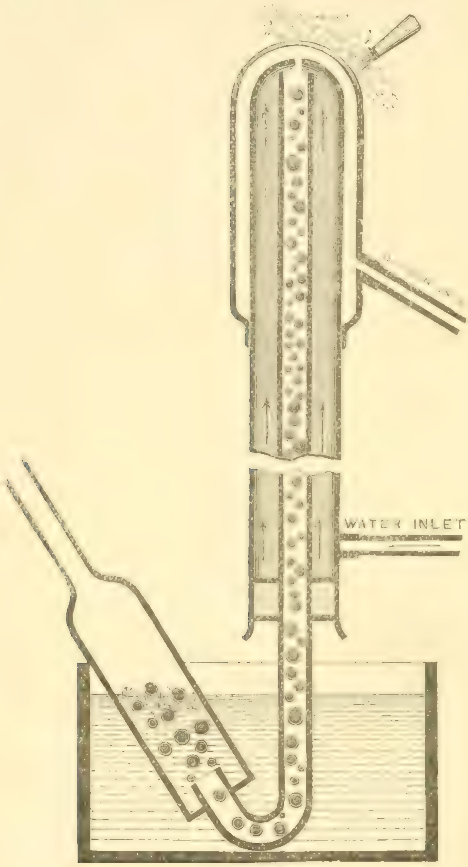
When the current of highly attenuated air blows downwards through the vacuum tube (which is surrounded by a box to prevent any light being seen from the electrical discharge) a luminous glow,

about two feet in length, resembling to the eye the tail of a comet, appears in the large glass vessel below. This phosphorescent glow is connected in some way with ozone, as it occurs only with oxygen compounds; impure air is fatal to success in the experiments, the glow being very sensitive to traces of organic matter, especially to the vapour of essential oils and substances which have a smell. Hydrogen extinguishes the light; pure oxygen increases it, and makes the glow shorter, with a tendency to break up at the lower end; carbonic acid gives a glow not so bright as air, and ozone is produced from its decomposition. Nitrous oxide gives a very bright brush. The phosphorescence disappears at once when a pocket-handkerchief containing any odoriferous matter is brought near the air inlet, and afterwards much time is lost in getting the apparatus to work as before. It is no easy matter to get the brushes back again; this was first found out accidentally—for days and weeks they had been puzzled in the laboratory to understand why one tube worked better than another. Bisulphide of carbon is an organic body, and is the only one, so far discovered, which allows the glow to be obtained in its presence. That these downward luminous brushes contain ozone is proved by means of the iodide of potassium starch test (and others), which darken in the brushes, but are not acted upon when placed outside them near the inner sides of the lower glass vessel. By suddenly altering the rate of oxygen supply passing through the vacuum tube most beautiful effects of apparent explosions of phosphorescent glows can be produced. It is remarkable that the rate of oxygen or air supply can be so regulated that the luminous emission seems to come from a steady current of gas passing down the middle of the tube, of almost uniform diameter, and blending very little with the surrounding space.

It is usually supposed that ozone is destroyed by heat, and can only be produced at a low temperature, yet in these vacuum tubes it is produced at a white heat. The piece of apparatus represented in Fig. 2 enables the chemist to demonstrate that ozone can be continuously produced by heating pure oxygen to a temperature of about 1600° C. The apparatus consists of a glass tube bent at its lower end, and passing up the centre of a tube of platinum; a little hole in the latter is placed just above the top orifice of the glass tube. The upper part of the apparatus is covered with an outer tube of platinum, which at the top very nearly touches the inner one. In action a regulated current of water flows up the inner platinum tube, then passes down the central glass tube, which is made longer than the platinum tube; consequently it sucks in and carries down air, which it draws through the little hole in the top of the inner platinum tube. The top of the outer tube is then raised to near the melting-point of platinum, by means of an oxyhydrogen flame, and the oxygen beneath raised to this temperature is suddenly withdrawn and cooled by the water current, and carried down to a collecting vessel for examination. When tested, it is found to contain ozone; hence ozone

has two centres of stability, and one of them is at about the melting-point of platinum. In this experiment ozone is formed by the action of a high temperature, owing to the dissociation of the oxygen molecules and their partial recombination into the more complex

Fig. 2.



Ozone Apparatus.

molecules of ozone. We may conceive it not improbable that some of the elementary bodies might be formed somewhat like the ozone in the whole experiment, but at very high temperatures, by the collocation of certain dissociated constituents and with the simultaneous absorption of heat.

[J. D.]

WEEKLY EVENING MEETING,

Friday, February 8, 1889.

Sir Frederick Bramwell, Bart. D.C.L. F.R.S. Honorary Secretary and Vice-President, in the Chair.

Sir William Thomson, D.C.L. LL.D. F.R.S. *M.R.I.*

Electrostatic Measurement.

A fundamental requisite of a measuring instrument is that its application to make a measurement shall not alter the magnitude of the thing measured. When this condition is not fulfilled (as is essentially the case with an electric measuring instrument not kept permanently in or on the electric circuit or system to which it is applied), it is the magnitude as influenced or modified by the measuring instrument which is actually measured, and the measurement is to be interpreted on this understanding, whatever may be the circumstances.

The nearest approach in electric measuring instruments to the fulfilment of this condition, of not altering the magnitude of the thing measured, is attained by the electrometer when applied to measure differences of potential between different points of a wire, or metallic mass of any shape, in which electricity is kept flowing by a battery or dynamo or other electromotive apparatus. The insulation of any practical electrometer is so nearly perfect that the conduction of electricity through the instrument does not sensibly diminish the difference of potentials of the points touched by the electrodes, and the consumption of energy is therefore practically *nil*. In this respect, therefore, the quadrant electrometer would be ideally perfect: but it is only available for potentials of a few volts, and in its most sensitive adjustment indicates about $\frac{1}{100}$ of a volt. The lecturer has therefore designed for ordinary use in connection with electric lighting and the other practical applications of electric energy, a series of instruments which will measure by electrostatic force potentials of from 40 volts to 50,000 volts. The construction of the various types of this series was fully explained.

The standardisation of these instruments up to 200 or 300 volts is made exceedingly easy, by aid of his centiampère balance and continuous rheostat, with a voltaic battery of any kind, primary or secondary, capable of giving a fairly steady current of $\frac{1}{10}$ of an ampère through it and the platinoid resistance in series with it. The accuracy of the electro-magnetic standardisation, within the range of the direct application of this method, is quite within $\frac{1}{20}$ per cent. A method of multiplication by aid of condensers, which was explained, gives an accuracy quite within $\frac{1}{5}$ per cent. for the measurement in volts up to

2000 or 3000 volts; and with not much less accuracy, by aid of an intermediate electrometer, up to 50,000 volts.

He also explained, and illustrated by a drawing, an absolute electrometer which he had constructed for the purpose of measuring "*v*," the number of electrostatic units of potential or electromotive force in the electro-magnetic unit of potential. This number "*v*" is essentially a velocity, and experiments have proved it to be so nearly equal to the velocity of light that from all the direct observations hitherto made we cannot tell whether it is a little greater than, or a little less than, or absolutely equal to, the velocity of light.

The determination was made by comparing the electro-magnetic with the electrostatic value, in C. G. S. units, as given by the balance, for a potential of 10,000 volts: but hitherto he has not been able to make sure of the absolute accuracy of the electrostatic balance to closer than $\frac{1}{2}$ per cent.

The results of a great number of measurements which had been made in the Physical Laboratory of the University of Glasgow during the previous two months gave the required number, "*v*," within $\frac{1}{2}$ per cent. of 300,000 kilometres per second; the velocity of light is known to be within $\frac{1}{4}$ per cent. of 300,000 kilometres per second. Results of previous observers for determining "*v*" had almost absolutely proved at least as close an agreement with the 300,000,000 metres. He expressed his obligations to his assistants and students in the Physical Laboratory of Glasgow University, Messrs. Meikle, Shields, Sutherland, and Carver, who worked with the greatest perseverance and accuracy, in the laborious and often irksome observations by which he had attempted to determine "*v*" by the direct electrometer method, as exactly as, or more exactly than, it has been determined by other observers and other methods.

Note added March 14*th*, 1889.

The measurement of "*v*" by Sir William Thomson and Profs. Ayrton and Perry, communicated to the British Association at Bath, was too small (292) on account of the accidental omission of a correction regarding the effective area of the attracted disk in the absolute electrometer. When this correction is applied their result is brought up to 298, which exactly agrees with Profs. Ayrton and Perry's previous determination by another method, in Japan. Prof. J. J. Thomson's result is 296·3. It is understood that Rowland has found 299. The result of Sir William Thomson's latest observations, founded wholly on the comparison of electrometric and electromagnetic determinations of potential in absolute measure, is 30·1 legal ohms, or 30·04 Rayleigh ohms. Assuming, as is now highly probable, that the Rayleigh ohm is considerably nearer than the legal ohm to the true ohm, the result for "*v*" is 300,400,000 metres per second. Sir William does not consider that this result can be trusted as demonstrating the truth within $\frac{1}{3}$ per cent.

GENERAL MONTHLY MEETING,

Monday, July 1, 1889.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

Robert Kaye Gray, Esq.
James Mactear, Esq. F.R.S.E. F.C.S.
William James Russell, Esq. Ph.D. F.R.S. Pres. C.S.
James Miln Small, Esq.

were elected Members of the Royal Institution.

The following Letter from Mrs. Henry Pollock was read:—

"Hall Place, Cranleigh, Guildford,
"*June 11th*, 1889.

"My Dear Sir Frederick,

"Although you ask me not to acknowledge it, I cannot refrain from answer-"ing your very kind letter, and thanking you and my husband's colleagues at the "Royal Institution with all my heart for the warm expression of the regard and "appreciation felt for him contained in the resolution and in your letter. The "interest my husband felt in the Institution was very deep. It was his greatest "pride and pleasure to be your Treasurer, and to do all he could to the last. I "am rejoiced to know how affectionately he was regarded by those with whom he "worked and for whom he cared so much. The interest he felt in the Royal "Institution was shared and always will be by my daughter and myself, and we "are deeply grateful for the sympathy now felt for us in our sorrow. May I "ask you to convey my earnest thanks to the Board, and thanking you again "most truly for your letter,

"Believe me,
"Very sincerely yours,
"Amelia Pollock."

Sir James Crichton Browne, M.D. LL.D. F.R.S. was elected Treasurer of the Royal Institution in the room of Henry Pollock, Esq. deceased.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XXII. Part 2. 4to. 1889.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1° Semestre, Vol. V. Fasc. 4, 5. 8vo. 1889.
Memorie, Vols. III. and IV. 4to. 1886–7.
Asiatic Society of Bengal—Journal, Vol. LVI. Part 2, No. 5; Vol. LVII. Part 2, No. 4. 8vo. 1888–9.
Proceedings, 1889, Parts 9, 10. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XLIX. No. 7. 8vo. 1889.
Bankers, Institute of—Journal, Vol. X. Part 6. 8vo. 1889.
British Architects, Royal Institute of—Proceedings, 1888–9, Nos. 15, 16. 4to.
California, University of—Publications, 1881–9. 8vo.
Chemical Industry, Society of—Journal, Vol. VIII. No. 5. 8vo. 1889.
Chemical Society—Journal for June, 1889. 8vo.
Civil Engineers' Institution—Proceedings, Vol. XCVI. 8vo. 1888–9.

Cornwall Polytechnic Society, Royal—Annual Report for 1888. 8vo. 1889.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, 1889, Part 3. 8vo.
East India Association—Journal, Vol. XXI. No. 2. 8vo. 1889.
Editors—American Journal of Science for June, 1889. 8vo.
Analyst for June, 1889. 8vo.
Athenæum for June, 1889. 4to.
Chemical News for June, 1889. 4to.
Chemist and Druggist for June, 1889. 8vo.
Electrical Engineer for June, 1889. fol.
Engineer for June, 1889. fol.
Engineering for June, 1889. fol.
Horological Journal for June, 1889. 8vo.
Industries for June, 1889. fol.
Iron for June, 1889. 4to.
Ironmongery for June, 1889.
Murray's Magazine for June, 1889. 8vo.
Nature for June, 1889. 4to.
Photographic News for June, 1889. 8vo.
Revue Scientifique for June, 1889. 4to.
Telegraphic Journal for June, 1889. 8vo.
Zoophilist for June, 1889. 4to.
Electrical Engineers, Institution of—Journal, No. 80. 8vo. 1889.
Franklin Institute—Journal, No. 762. 8vo. 1889.
Geographical Society, Royal—Proceedings, New Series, Vol. XI. No. 6. 8vo. 1889.
Georgofili, Reale Accademia—Atti, Vol. XII. Disp. 1. 8vo. 1889.
Linnean Society—Journal, No. 170. 8vo. 1889.
Mechanical Engineers' Institution—Proceedings, 1889, No. 1. 8vo.
Meteorological Office—Weekly Weather Reports, Nos. 23, 24, 25. 4to. 1889.
Meteorological Society, Royal—Quarterly Journal, No. 70. 8vo. 1889.
Meteorological Record, No. 32. 8vo. 1889.
Ministry of Public Works, Rome—Giornale del Genio Civile, Serie Quinta, Vol. III. Nos. 3, 4. 8vo. And Disegni. fol. 1889.
Mitchell, C. Pitfield, Esq. (the Author)—Dissolution and Evolution, and the Science of Medicine. 8vo. 1888.
Perry, Rev. S. J. F.R.S.—Results of Meteorological and Magnetical Observations for 1888. 8vo. 1889.
Pharmaceutical Society of Great Britain—Journal, June, 1889. 8vo.
Photographic Society—Journal, Vol. XIII. No. 8. 8vo. 1889.
Radcliffe Observatory—Observations for 1885. 8vo. 1889.
Richardson, B. W. M.D. F.R.S. M.R.I. (the Author)—The Asclepiad, Vol. V. No. 22. 8vo. 1889.
Rio de Janeiro Observatory—Revista, No. 5. 8vo. 1889.
Royal Historical and Archæological Association of Ireland—Journal, Vol. IX. (4th Series) No. 78. 8vo. 1889.
Royal Society of London—Proceedings, No. 279. 8vo. 1889.
Rust, Arthur, Esq. (the Author)—Thermo-Electricity. 8vo. 1889.
Saxon Society of Sciences, Royal—Mathematisch-physische Classe:
Abhandlungen, Band XV. No. 5. 8vo. 1889.
Berichte, 1889, No. 1. 8vo. 1889.
Society of Arts—Journal for June, 1889. 8vo.
United Service Institution, Royal—Journal, No. 148. 8vo. 1889.
Upsal University—Bulletin de l'Observatoire Météorologique, Vol. XX. 4to. 1888–9.
Veneto, L'Ateneo—Rivista, Sept. 1887 to Oct. 1888. 8vo.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1889: Heft 5. 4to.
Zoological Society—Proceedings, 1889, Part 1. 8vo.

GENERAL MONTHLY MEETING.

Monday, November 4, 1889.

SIR JAMES CRICHTON BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

The Honorary Secretary announced that His Grace The President had nominated Lord Arthur Russell a Vice-President for the ensuing year.

Thomas Browning, Esq. C.B.
Latimer Clark, Esq. F.R.S. M. Inst. C.E.
Charles Pitfield Mitchell, Esq. M.R.C.S.
Maurice Powell, Esq. M.A.
Delisle Powles, Esq.

were elected Members of the Royal Institution.

EDWARD POLLOCK, Esq. was elected a Manager of the Royal Institution in the room of SIR JAMES CRICHTON BROWNE, M.D. resigned; and JAMES EDMUNDS, M.D. F.C.S. was elected a Visitor in the room of EDWARD POLLOCK, Esq. resigned.

The Special Thanks of the Members were returned to Professor DEWAR, F.R.S. *M.R.I.* for his valuable present of a Portrait of the late Mr. Henry Pollock.

The Special Thanks of the Members were returned to the Rev. JOHN MACNAUGHT, *M.R.I.* for his second donation of £50 for improvements in the Building.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Governor-General of India—Geological Survey of India: Records, Vol. XXII. Part 3. 4to. 1889.
The French Government—Documents Inédits sur l'Histoire de France: Lettres du Cardinal Mazarin. Par A. Chéruel. Tome V. 1653. 4to. 1889.
Academy of Natural Sciences, Philadelphia—Proceedings, 1889, Part 1. 8vo.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quarta: Rendiconti. 1o Semestre, Vol. V. Fasc. 7–12. 8vo. 1889.
American Association for the Advancement of Science—Proceedings, 37th Meeting held at Cleveland, 1888. 8vo. 1889.
American Philosophical Society—Proceedings, Vol. XXVI. No. 129. 8vo. 1889.
Antiquaries, Society of—Proceedings, Vol. XII. No. 3. 8vo. 1889.
Asiatic Society, Royal (China Branch)—Journal, Vol. XXIII. Parts 2, 3. New Series. 8vo. 1889.

Astronomical Society, Royal—Monthly Notices, Vol. XLIX. No. 8. 8vo. 1889.
Australian Museum, Sydney—Report for 1888. 4to. 1889.
Bankers, Institute of—Journal, Vol. X. Parts 7, 8. 8vo. 1889.
Bavarian Academy of Sciences—Abhandlungen, Band XVI. Abth. 3. 4to. 1889.
Sitzungsberichte, 1888, Heft 3; 1889, Heft 1. 8vo. 1889.
Belgique, Académie des Sciences, &c.—Mémoires, Tome XLVII. 4to. 1888.
Mémoires couronnés et des savans étrangers, Tome XLIX. 4to. 1889.
Mémoires couronnés et autres mémoires, Tomes XL.–XLII. 8vo. 1889.
Bulletins de l'Académie, Tomes XIV.–XVII. 8vo. 1889.
Annuaires de 1888 et 1889. 8vo. 1889.
Birmingham Philosophical Society—Proceedings, Vol. VI. Part 1. 8vo. 1889.
British Architects, Royal Institute of—Proceedings, 1888–9, Nos. 17–20. 4to.
Calendar, 1889–90. 8vo. 1889.
British Museum (Natural History)—Catalogue of Fossil Reptilia and Amphibia, Part 2. 8vo. 1889.
Catalogue of Hindustani Printed Books. By J. F. Blumhardt. 4to. 1889.
California, University of—Register, 1888–9. 8vo.
Canadian Institute—Proceedings, 3rd Series, Vol. VI. Fas. 1. 8vo. 1889.
Chemical Industry, Society of—Journal, Vol. VIII. Nos. 7–9. 8vo. 1889.
Chemical Society—Journal for July–October, 1889. 8vo.
Chief Signal Officer, U.S. Army—Annual Report for 1888. 8vo. 1889.
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Colonial Institute, Royal—Proceedings, Vol. XX. 8vo. 1889.
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GENERAL MONTHLY MEETING,

Monday, December 2, 1889.

SIR JAMES CRICHTON BROWNE, M.D. LL.D. F.R.S. Treasurer and Vice-President, in the Chair.

Charles Vernon Boys, Esq. F.R.S. A.R.S.M.
Frederick Bayard Wiggins, Esq.
Alfred Fernandez Yarrow, Esq. M. Inst. C.E.

were elected Members of the Royal Institution.

The MANAGERS reported that they had re-appointed Professor JAMES DEWAR, M.A. F.R.S. as Fullerian Professor of Chemistry.

The following Lecture Arrangements were announced:—

PROFESSOR A. W. RÜCKER, M.A. F.R.S. *M.R.I.* Professor of Physics in the Normal School of Science and Royal School of Mines, South Kensington. Six Lectures (adapted to a Juvenile Auditory) on ELECTRICITY. On Dec. 28 (*Saturday*), Dec. 31, 1889; Jan. 2, 4, 7, 9, 1890.

GEORGE JOHN ROMANES, Esq. M.A. LL.D. F.R.S. *M.R.I.* Fullerian Professor of Physiology, R.I. Ten Lectures, constituting the third part of a Course on BEFORE AND AFTER DARWIN (The Post-Darwinian Period). On *Tuesdays*, Jan. 21 to March 25.

EDWIN ROSCOE MULLINS, Esq. Three Lectures on SCULPTURE IN RELATION TO THE AGE. On *Thursdays*, Jan. 23, 30, Feb. 6.

THE REV. CANON AINGER, M.A. LL.D. Three Lectures on THE THREE STAGES OF SHAKSPEARE'S ART. On *Thursdays*, Feb. 13, 20, 27.

FREDERICK NIECKS, Esq. Author of the 'Life of Chopin.' Four Lectures on THE EARLY DEVELOPMENTS OF THE FORMS OF INSTRUMENTAL MUSIC (with Musical Illustrations). On *Thursdays*, March 6, 13, 20, 27.

PROFESSOR FLOWER, C.B. D.C.L. LL.D. F.R.S. Three Lectures on THE NATURAL HISTORY OF THE HORSE AND OF ITS EXTINCT AND EXISTING ALLIES. On *Saturdays*, Jan. 25, Feb. 1, 8.

THE RIGHT HON. LORD RAYLEIGH, M.A. D.C.L. LL.D. F.R.S. *M.R.I.* Professor of Natural Philosophy, R.I. Seven Lectures on ELECTRICITY AND MAGNETISM. On *Saturdays*, Feb. 15, 22, March 1, 8, 15, 22, 29.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Nature for November, 1889. 4to.
Photographic News for November, 1889. 8vo.
Revue Scientifique for November, 1889. 4to.
Telegraphic Journal for November, 1889. 8vo.
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WEEKLY EVENING MEETING,

Friday, March 29, 1889.

William Crookes, Esq. F.R.S. Vice-President, in the Chair.

A. Gordon Salamon, Esq. A.R.S.M. F.I.C. F.C.S. *M.R.I.*

Yeast.

"For the first time in the history of the science, we are justified in entertaining the sanguine, nay the certain hope, that medicine, so far as epidemic illnesses are concerned, will soon be rescued from empiricism and placed upon a sound scientific basis. When this great day shall arrive, mankind will, in my opinion, acknowledge that to you is due the greatest share of its gratitude."*

These words were penned in this building by Professor Tyndall just thirteen years ago, and were addressed to M. Pasteur. The reflection that they had reference mainly to researches which the great French investigator had carried out during several years in connection with the subject of this evening's discourse, will suffice to constitute for it a sufficient claim upon your attentive consideration.

It will be within the knowledge of those who have closely followed the work of Pasteur, and have noted how contracted is the circle of actual experiment, how expanded—yet how accurately traced—the sphere of legitimate induction, that the results arising out of his examination into the nature and properties of yeast constituted the weapons which demolished the mischievous doctrine of spontaneous generation, fashioned the modern views respecting putrefactive decomposition, and prepared the way for the prosecution of those investigations which have already revolutionised the theory and practice of Hygiene, Therapeutics, and Surgery.

All this was not effected without concomitant controversy of the keenest kind. Indeed, the protracted dispute which Pasteur sustained with Liebig, and also with Frémy, regarding the rationale of the decomposition of sugar solutions by yeast must always rank among the most brilliant of his efforts.

But, perhaps, the most interesting feature in connection with his experiments upon yeast is that his abstruse physiological researches should have been capable of direct translation into terms of industrial utility; and but for other work which he himself has done in this direction, and but for that of Hansen, which I propose to consider this evening, it would probably be unique in the history of technology.

As regards the value of that work, when considered from a com-

* M. L. Pasteur, 'Études sur la Bière,' p. 382. Paris, Gauthier-Villars, 1876.

mercial standpoint, it may, so far as this country alone is concerned, most undoubtedly be estimated by millions sterling; and when all those problems which have arisen out of the investigation—and which have of necessity been taken in hand by others—shall have been solved, it will be found that the saving due to it will have reached a total which, in terms of pounds, shillings, and pence, would appear truly colossal.

For this to be possible, yeast must have a very important rôle to perform in connection with certain of our industries, and indeed *is* used in quantity which is really somewhat astonishing when the figures are cast up for the first time.

I have endeavoured, with the assistance of my friends, Mr. Bannister, Deputy Principal of the Laboratory of Somerset House, and Mr. Frank Wilson, the head brewer to Messrs. Combe and Co., to calculate the amount of yeast (reckoned as pressed dry yeast) which is annually produced and used in the United Kingdom. The results are given in the following table, the figures of which, though in some cases necessarily estimated, may, I think, be regarded as very nearly accurate.

YEAST ANNUALLY USED AND PRODUCED IN THE UNITED KINGDOM.
(Calculated as Pressed Yeast.)

In Brewing—	Tons.	
Used for "Pitching"	4,250	
Produced in addition to above *	25,500	Tons.
		29,750
In Distilling—		
Used for "Pitching"		3,100
In Vinegar-making—		
Used in "Pitching" and produced		125
In Baking—		
Used for making "Sponge" †		14,000
Total		46,975

* Mainly employed for use in foreign distilleries, and having value to brewers of about £127,500

† Average imports of foreign yeast especially grown and prepared for use in baking, 12,594 tons, having declared average value to importer of £684,156

Approximate amount annually paid by merchants for yeast used in the United Kingdom £811,500

Now this great quantity of yeast—and when we regard yeast, as we shall do here, as a cultivated fungus, it is a great quantity—is used in virtue of the property it possesses of decomposing certain of the carbohydrates into alcohol and carbonic acid.

In the production of beer and spirit it is employed with the object of producing alcohol; in vinegar-making with the same object, the alcohol being subsequently oxidised by means of another organism into acetic acid. In each of these cases the carbonic acid gas is a valueless bye-product; but in bread-making the conditions are reversed, the yeast being primarily employed with a view to the dis-

engagement of carbonic acid gas. It is through its agency that the production of a light and vesicular bread is ensured. The yeast being brought into intimate contact with carbohydrates suitable for decomposition, effects the generation of carbonic acid gas, which, as it is disengaged, makes an effort to force its way through the dough. This is resisted by the very plastic gluten of the flour. The result is to effect what is termed the raising of the "sponge."

It will be unnecessary to remind you that this decomposition of carbohydrates into alcohol and carbonic acid gas by means of yeast constitutes one phase of the phenomenon of alcoholic fermentation. It is somewhat difficult to ascertain when it was first conjectured that this was due to the growth of yeast; but it is clear that Caignard de la Tour had formed a strong opinion on the subject more than sixty years ago, for he wrote, "if yeast could thus ferment sugar, it must surely be by some influence due to its vegetation and its life." Indeed, there are many passages in his works, as well as in those of that great observer, Schwann, which indicate that about that period a very firm grip had been obtained of the theory of fermentation as now accepted; but it had to be relinquished, as it clashed with the more speculative views of Liebig and his supporters. So widespread was the influence of this great school, that operations and experimental observations contradictory of its doctrines were so neglected as to be forgotten, or were combated by so strong a combination of intellect as to be practically untenable.

It was reserved for Pasteur to rediscover the growth of yeast previously noted by Caignard de la Tour and Schwann; to add to their observations by an abundance of fresh investigations; and to formulate a theory of alcoholic fermentation, which has at least the merit, in contradistinction to others, of being a logical interpretation of accurate experiment; one, moreover, which, in my humble judgment, is quite capable of ultimate reconciliation with some at least of the more important views of the German chemist.

Pasteur's experiments placed it beyond doubt that the decomposition of carbohydrates by means of yeast is neither more nor less than one of the manifestations of vitality by yeast, a fungoid organism capable of growth and reproduction. Since his results have been published it has been almost proved that this decomposition takes place within the organism, and in any case it is proved that alcohol and carbonic acid are to be regarded as products excreted during phases of its life-history. This being the case, it manifestly becomes necessary to study the biology of the organism, in order to acquire further information respecting the nature of the chemical reactions involved in the decomposition.

But before doing this, it is advisable, at any rate for the purposes of this discourse, to restrict the signification of the term ferment; otherwise confusion must inevitably result. Even to-day we are accustomed to speak of two classes of ferments—organised and soluble.

The alcoholic ferment, yeast, for example, is a cryptogam of definite

organised structure, and its power of effecting the degradation of carbohydrates into alcohol and carbonic acid gas is but one of the features connected with its life-history. Moreover, it is one which is typical of a whole group of fungi.

The soluble ferments, on the other hand, are unable to effect this radical decomposition, but *are* endowed with the property of hydrolising, and thereby producing a constitutional modification or partial degradation of some carbohydrates and other substances of complex composition. They are probably typified by certain molecular groupings common to them as a class, but they are not organised, and have indeed nothing beyond what I have stated in common with fungi. It is true that certain of these soluble ferments are contained in fungi which themselves are capable of producing alcoholic fermentation, such, for instance, as the invertase in yeast; but they are not contained in all alcohol-producing fungi. Their function, when present, would seem to be that of a reserve material, capable, when necessary, of effecting the preparation of a saprophytic food by alterative action, so as to adapt it for subsequent radical attack by the fungus. In this connection it must not be forgotten that sulphuric acid, hydrochloric acid, and indeed most mineral and organic acids, can produce the same alterative action upon these carbohydrates, and are employed in very large manufacturing operations for the purpose. It would therefore seem quite reasonable to include these mineral acids among the so-called soluble ferments, if the latter are entitled to rank as a distinct class.

Alcoholic fermentation, although caused by the action of yeast, which is a fungus, is by no means peculiar to one particular species or genus of fungi. Indeed, the list of those organisms which, under certain conditions, will excite alcoholic fermentation, is continually being augmented. But the life conditions are now tolerably well understood. The fungal food must be present in a state of solution; the supply of free oxygen must be extremely restricted; the temperature must be maintained within certain limits for each particular species or variety; there must be present an adequate but not too great a supply of carbohydrate, preferably a sugar, in solution; and the fungus must, when immersed in the fluid, be capable of growth and reproduction.

With regard to the carbohydrate food, some points of very great interest are noticeable. All carbohydrates are not equally suitable, though in most cases the fungus contains within itself the chemical components that may, when required, adapt it to assimilation. Ordinary yeast, for instance, cannot ferment cane sugar; it has, first of all, to be transformed, by the invertase contained in yeast, or by other known means, into what is known as invert sugar. This consists of two glucoses present in equal quantity—the one dextrose, capable, as its name implies, of turning the ray of polarised light to the right; the other, lævulose, capable of rotating it to a definite extent to the left. Both these glucoses have the same composition;

but there is probably a difference in their molecular grouping which influences their respective actions towards polarised light. If ordinary yeast—a member of the family of sprouting fungi—be submerged, under proper conditions, in a mixture of equal quantities of these two glucoses, it will be found that it is capable of distinguishing the one from the other, and of exercising a selection with respect to assimilation; for it will entirely decompose the dextrose before attacking the lævulose. Indeed, if the fermentation be stopped at the proper point, it is a very good way of obtaining lævulose. This is by no means the only case in point, as will be seen by reference to the following table:—

Selective Action of some Sprouting Fungi for Carbohydrate Food.

Saccharomyces cerevisiæ ..	Can invert cane sugar, and subsequently select dextrose from lævulose. Can ferment maltose. Cannot decompose malto-dextrin.
" pastorianus .. " ellipsoideus ..	Can invert sugar, ferment maltose, and decompose malto-dextrin.
" exiguus	Cannot ferment maltose. Can ferment glucose.
" apiculatus ..	Cannot ferment maltose. Cannot invert cane sugar. Can ferment dextrin and lævulose.
Torula	Can ferment maltose and glucose. Cannot invert cane sugar.
Monilia candida	Cannot invert cane sugar, but can ferment it without previous conversion into glucose.

In order that yeast, or any fungus capable of exciting alcoholic fermentation, may flourish and reproduce, it is necessary that it shall have an available supply of oxygen. This has to discharge a duty which Pasteur deems analogous to respiration. If it is deprived of oxygen, and is not introduced into a fermentable medium, the yeast will not reproduce, but in a short time will become weak and shrivelled; but if, before it is too late, a supply of oxygen be introduced, the exhausted organism will revive and again be capable of manifesting all its vitality.

If the alcoholic ferment be deprived of a sufficiency of oxygen, and in such circumstances be kept immersed in a medium containing suitable carbohydrate in solution, it will decompose the latter, in order, among other things, to obtain the necessary oxygen. This decomposition will result in the production of alcohol and carbonic acid gas in strictly definite proportions.

$$\underset{\text{(Glucose)}}{2C_6H_{12}O_6} = \underset{\text{(Alcohol)}}{4C_2H_6O} + \underset{\text{(Carbonic anhydride)}}{4CO_2}.$$

On looking at this equation, one fails to see the available oxygen, although it may be that the decomposition itself suffices for the requirements of what has been termed "the respiration" of the yeast. But what is more likely is, that it is furnished by other decompositions by which the reaction is always attended. Thus Pasteur has proved that in addition to alcohol and carbonic anhydride,

small quantities of succinic acid and glycerin always accompany their formation, and in attempting to state this in the form of an equation, Monoyer satisfactorily accounts for the liberation of oxygen. Thus—

$$\underset{\text{(Glucose)}}{4(C_6H_{12}O_6)} + 3H_2O = \underset{\text{(Succinic Acid)}}{C_4H_6O_4} + \underset{\text{(Glycerin)}}{6C_3H_8O_3} + 2CO_2 + O.$$

Various opinions have been expressed as to whether the presence or absence of oxygen does actually influence the decomposition of sugar and the formation of yeast to the extent indicated by Pasteur. Adolf Mayer, whose opinion upon questions connected with fermentation is justly regarded with great respect, made a series of experiments from which he inferred that the influence was by no means as great as was supposed; but Schützenberger pointed out what certainly looks like a flaw in his method of working, and would induce hesitancy in accepting his conclusions. The great difficulty is to ensure the complete permeation of the fermenting fluid by oxygen, which has a tendency, if not corrected, to exercise its influence only upon layers or strata in the fluid. It is found that the rate of fermentation greatly influences the depth to which oxygen will permeate and diffuse, and that a slow fermentation favours efficient admixture. These conditions have been brought under control by Hoppe-Seyler in a most ingenious and satisfactory manner.* His experiments upon the point in question may be thus stated and tabulated:—

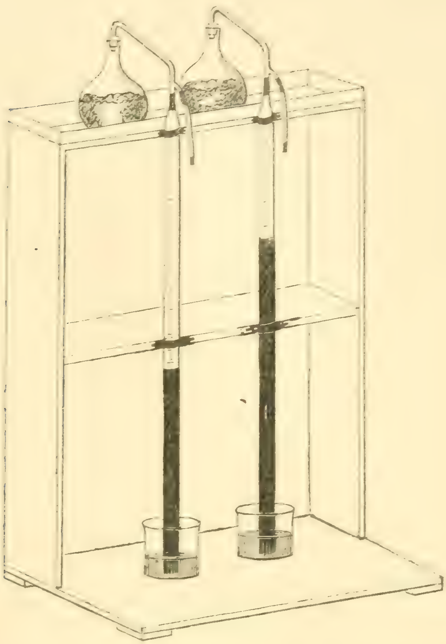
Experiment.	Units of Sugar decomposed.	Units of Alcohol formed.	Remarks.
1. CO_2 absorbed and replaced by O uniformly diffused.	1	1	Few budding cells; cell contents very granular; alcoholic distillate *strongly* acid.
2. Air above fermenting fluid replaced by CO_2, which was also passed in during fermentation instead of O.	1·5	2·05	Cell contents normal; alcoholic distillate *feebly* acid.
3. Normal fermentation	2·74	4·70	Cell contents normal; alcoholic distillate *faintly* acid.

Now, from what has been said it will be understood, indeed it follows, that the amount of carbonic anhydride evolved is a measure of the extent to which the decomposition of the carbohydrate has been carried by the alcoholic ferment. But, in order that this statement may hold good, it is necessary that whenever the carbonic anhydride is measured, there shall always be present in the fermenting fluid an excess of the solution of carbohydrate; for Pasteur

* Festschrift von Felix Hoppe-Seyler. Karl J. Trübner, Strasburg, 1881.

has proved that, in its absence, the yeast will, when denied free access of atmospheric oxygen, and when fermentation has once started, decompose a portion of itself in order to obtain the necessary elements to maintain its vitality. But given the necessary excess of carbohydrate, and assuming that in two or more flasks undergoing alcoholic

Fig. 1.

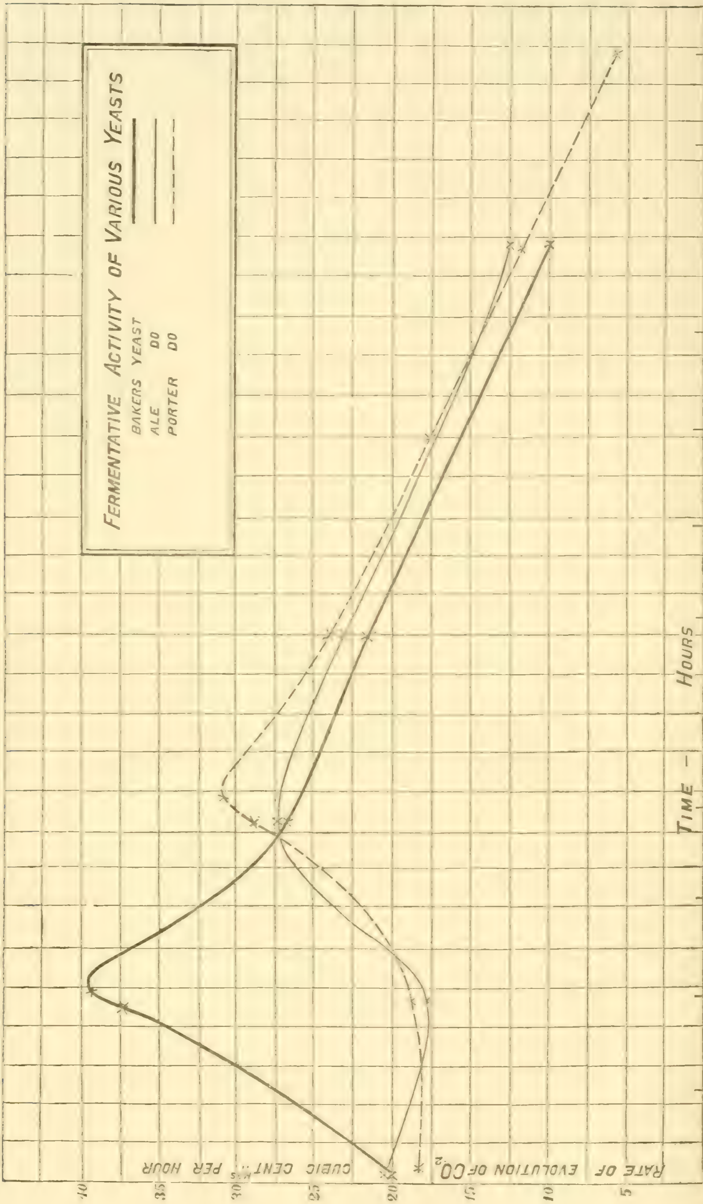


fermentation the conditions of culture are the same with respect to temperature, pressure, food, and amount of ferment added, it follows that the rate of evolution of carbonic anhydride becomes a measure of the fermentative activity of the fungus respectively producing it. I have here an apparatus devised to effect this measurement. It will be seen to be extremely simple in its working, but I find that it gives results of surprising constancy.

As introductory to the working of this apparatus, I will ask your permission to repeat the well-known experiment which proves that carbonic anhydride is one of the resulting products of alcoholic fermentation. [Experiment shown.]

The two flasks which I have here, contain (represented in the diagram) equal quantities of the same sugar solution and of the

Fig. 2.


FERMENTATIVE ACTIVITY OF VARIOUS YEASTS
BAKERS YEAST
ALE DO
PORTER DO
TIME — HOURS
0
12
24
36
48
60
72
84
96
108
120
132
144
RATE OF EVOLUTION OF CO2 CUBIC CENT.s PER HOUR
40
35
30
25
20
15
10
5

nitrogenous material which is best calculated to promote fermentation. To each respectively has been added equal weights of two different kinds of yeast. To one has been added yeast used in the making of bread, and to the other that employed in the production of porter. The temperature of the fermenting fluid has been the same in each case, likewise the time in which fermentation has been progressing. The two columns of coloured liquid were filled a short time previous to the commencement of the discourse, and the extent to which they have been respectively depressed is a measure of the amount of carbonic anhydride which has in each case been evolved in the same time. You will notice that there is a marked difference in the two results.

The diagram of curves, which embodies the most typical of many experiments, and in which the abscissæ have been determined by experiment, as shown, renders this difference clearly apparent.

These results bring us to the heart of the question, and open up a vast field of inquiry, into which for lack of time we can this evening only travel a short distance. Granting that yeast is a plant—a fungus—may prompt the questions as to whether it be one genus, one species of a genus, or whether there be not many varieties of the species which can thus give rise to fermentations so different in character and vigour. To these questions the unrivalled researches of Hansen, of Copenhagen, have given an answer which leaves no room for hesitancy or doubt.

The power of inciting alcoholic fermentation is not confined even to one genus of fungi, for, although it is essentially typical of the group known as the sprouting fungi, as opposed to the more widely distributed hyphal fungi, still it is true of many of the latter, and Brefeld has proved it to be the case with all the mucorini. It has been asserted, moreover, that some bacteria are capable of decomposing sugar into alcohol and carbonic acid, thereby constituting themselves alcoholic ferments. This latter question, however, is one which demands further investigation.

Before considering the morphological distinctions of the different species and varieties of yeast which have been thus isolated and examined, it is well to make ourselves acquainted with the nature and properties of yeast, the latter being now considered as constituting a generic term. [Various kinds of yeast, as employed in the arts, were shown, and their morphological peculiarities, as revealed by the microscope, demonstrated.]

I am indebted to the great kindness of Dr. Hansen, which I desire most cordially to acknowledge, for preparations from pure cultures of all the species and varieties of yeast hitherto isolated. These I am enabled to render visible to you to-night, not in the form of drawings, but by means of lantern slides, which have been prepared by the skilled hands of Mr. Andrew Pringle from photographs which he himself took from the preparations furnished me by Dr. Hansen. [Slides exhibited, and the properties of the various specimens

explained and discussed.] The method of obtaining pure cultures of yeast organisms, as devised and perfected by Dr. Hansen, was next dealt with, and its industrial importance demonstrated. A full description of the process and of the apparatus required will be found in the Cantor Lectures delivered before the Society of Arts (January–February 1888) by the author.

[A. G. S.]

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